THE

MONTHLY

JOURNAL OF FOREIGN MEDICINE.

DECEMBER, 1828.

From the Transactions of the Medical and Chirurgical Society of London.

PATHOLOGICAL AND SURGICAL OBSERVATIONS RELATING TO INJURIES OF THE BRAIN. By B. C. Brodie, F. R. S. and Surgeon to St. George's Hospital.

[Continued from page 397.]

Sect. 7.—*Treatment of Concussion of the Brain.*

Although the treatment which is required in the first period which elapses after an injury of the head is neither various nor complicated, yet, in order that it should be conducted with advantage, it is necessary that many circumstances should be taken into consideration. We are called upon not only to do that which is to contribute to the relief of the present symptoms, but to guard against future ill consequences, and where no symptoms actually exist we are to look to those which may occur hereafter, and which proper measures of precaution may enable us to prevent or mitigate.

It is commonly remarked that two opposite methods of treatment have been recommended in cases of concussion of the brain; the one consisting of the exhibition of stimulants and cordials: the other comprising blood-letting, and what are usually termed antiphlogistic remedies. Here, however, as on many other occasions, the opposition of opinion is probably greater in appearance than in reality; and I am inclined to believe that if the advocates of the respective systems were questioned on the subject, it would be found that the views which they entertain are not essentially dissimilar. I suppose that none of those who have suggested the exhibition of stimulants would actually be inclined to apply this practice to cases in which the pulse has regained its strength and regularity; and, on the other hand, I conclude that no one among those who have advised the use of the lancet would think of taking away blood when the patient lies with pale cheeks, and cold extremities, and a feeble and intermitting pulse, or would refuse to resort to the cautious exhibition of cordials and stimulants where these symptoms are so urgent that he is manifestly in danger of sinking, in consequence of the depressed state of the circulation which has followed the first shock of the injury.

Cases of this last description are however in reality of rare occurrence: and there are indeed sufficient reasons why we should regard that condition of the system which approaches to syncope, as being, in the great majority of instances in which it exists, conducive to the patient's welfare, and why we should wish to prolong, rather than to abridge the period of its duration. The same blow which gives rise to symptoms of concussion frequently occasions the rupture of some small vessels within the cranium. The same state of the system which produces an enfeebled action of the heart is calculated to prevent the ruptured vessels from pouring out their contents; and the longer it continues, the less is the danger of internal hæmorrhage. If we artificially excite the action of the heart by the exhibition of wine and ammonia, we are in danger of inducing symptoms of pressure on the brain. If on the contrary we watch the gradual restoration of the pulse, and at the proper moment take from the arm a sufficient quantity of blood to prevent the heart resuming its wonted action, it is probable that we may often succeed in checking or arresting an extravasation of blood on the surface of the brain, or among its membranes, which might otherwise prove fatal. There is also the following very important circumstance which is not to be overlooked in this part of the inquiry. A state of depression is followed by a state of excitement. As the patient recovers from the former, the pulse, with respect to fulness and strength, becomes raised above the natural standard, and it is evident that this affords an additional argument in favour of the practice which is here recommended.

The same views respecting the prevention of internal hæmorrhage, which incline us to take blood from the arm in the first instance, cannot fail to influence our conduct afterwards. There is no evident reason why vessels, which have once bled, should not be liable to bleed again within the cranium, as well as in other situations. I have already mentioned a case in which a patient, who was apparently going on favourably, suddenly expired in consequence of such secondary hæmorrhage, on the fourth day after the occurrence of the in-

jury. If similar cases are rare, this may reasonably be attributed to the remedies which modern surgeons, with few exceptions, do not fail to employ. At any rate, where so much is at stake, we are called upon to neglect no measures of precaution; and however small the danger from this cause may really be, the surgeon should provide against it, by frequently inquiring into the state of his patient: by urging the necessity of continued repose of body and mind, by limiting him to a scanty vegetable diet, by the exhibition of laxative medicine, and by the abstraction of blood, whenever the state of the pulse indicates that this may be done with propriety.

Independently of the foregoing, there are other considerations which might of themselves lead us to adopt the same method of treatment. I believe that the patient in cases of concussion will generally spontaneously recover from that state of insensibility in which he remains after the vigour of the circulation is restored. But, nevertheless, from the best observations which I have made on the subject, I cannot doubt that his recovery is much assisted by repose and low diet, and depleting remedies. Often immediately after being bled, the patient, who before was in a state of stupor, exhibits manifest signs of returning sense. Further, it may be urged that concussion is liable to be followed by inflammation of the brain, or its membranes. Now I do not mean to say that such inflammation can always be prevented, or that the abstraction of very large quantities of blood will make the patient a better subject for it if it should occur; but it seems reasonable to suppose, and our experience of these cases, and other cases bearing an analogy to them, confirms the opinion, that there is less danger of inflammation, where the antiphlogistic treatment has been carried to a moderate extent, and where the patient has been kept in a state of perfect quiet, than where bleeding and laxative medicines have been neglected, and the patient has been allowed to exercise his body and mind, and to live on his usual diet.

The quantity of blood which the vessels of the brain contain depends very much on the position of the head with respect to the rest of the body. Not only in cases of concussion, but in all other cases where there has been an injury of the brain, or one likely to affect the brain, the head and shoulders should be raised by additional pillows, so that the blood may have an easy descent to the right side of the heart. In addition to this, in severe cases of concussion, the head should be shaved, and compresses should be applied constantly with a cold evaporating lotion. Opiates should be avoided. It is difficult to conceive what good purpose they can ever have been expected to answer; and, at any rate, they tend to constipate the bowels, and not unfrequently cause a confusion of symptoms, the patient complaining of headach, of which it is difficult to say whether it belongs to the injury itself or to the opium.

In taking a view of the various satisfactory reasons which may be urged in favour of a particular plan of treatment in cases of concussion of the brain, we must not overlook the circumstance that this treatment may be carried too far: and we must endeavour to avoid the error which I have known some surgeons fall into, of resorting to a too free use of the lancet. At first when the reaction of the heart has taken place, it may be right that the patient should lose a considerable quantity of blood, so as completely to subdue the force of the circulation. Afterwards, for the most part, it is only an occasional blood-letting that is required, and that to a moderate extent. It has appeared to me that this mode of proceeding has usually done more, both towards relieving the present symptoms, and preventing subsequent inflammation, than a more active system of depletion: and where very large quantities of blood have been already taken away, if inflammation should show itself, our resources are comparatively limited, and we are not able to meet it with that energy and vigour which the circumstances of the case require.

Where bleeding has been carried to a great extent, symptoms frequently occur which in reality arise from the loss of blood; but which a superficial observer will be led to attribute to the injury itself, and concerning which indeed it is sometimes difficult, even for the most experienced surgeon, to pronounce in the first instance to which of these two causes they are to be referred. Repeated copious blood-letting is of itself adequate to produce a hardness of the pulse, which we shall in vain endeavour to subdue by persevering in the same system of treatment. In many individuals it will produce headach and confusion of mind, not very different from what the injury itself had previously occasioned. These things may be observed especially in young females who are disposed to hysteria, and whom I have often known to suffer from a continued aggravation of such symptoms as I have described, while the system of depletion has been continued, recovering immediately on the use of the lancet being laid aside, and on their being allowed to take solid nourishment, with occasional doses of carbonate of ammonia.*

SECT. 8. *Treatment to be employed in cases of Compression of the Brain not complicated with wounds of the brain or its membranes.*

When we consider the variety of circumstances under which compression of the brain may follow an injury of the head, and the different effects which it produces in different instances, we cannot suppose that the same mode of treatment will be found applicable to all cases, or that any such simple rules can

* Dr. Marshall Hall has published, in the thirteenth volume of the Medico-Chirurgical Transactions, some excellent practical observations on the effects of copious blood-letting, many of which are applicable to the cases mentioned above.

be laid down for the conduct of the surgeon as those which we have to guide us in cases of concussion.

There is one most important complication which aggravates very much the ultimate danger, not only of these, but of all other cases of injury of the head; namely, the existence of a wound or laceration of the dura mater. This circumstance also tends to modify if not to alter the surgical treatment which is to be adopted. At present I suppose that such a complication does not exist; that the brain suffers from pressure, but that the dura mater is entire, and that there is no exposure of the important parts which are contained within it.

Where the symptoms of compression are such that the patient's life is manifestly in danger, there can be no question as to the propriety of removing the cause on which they depend, where that can be accomplished by means of a surgical operation.

In cases in which there is a fracture and depression of bone, it is generally in our power to remove or elevate the depression. If there be a wound of the scalp we may at once resort to the application of the trephine, or in some cases, where the cranium is not only fractured but splintered, we may do what is required by means of the forceps and elevator, without the aid of the saw. Where however the scalp remains entire, it will of course in the first instance be necessary to divide it, so that the bone may be completely exposed, and that the surgeon may be enabled to trace the extent of the mischief which has been inflicted on it.

An operation is also to be resorted to in those cases in which there are symptoms of pressure depending on hæmorrhage between the dura mater and the bone. But here another question arises: what is the evidence which is to enable us to detect a mass of extravasated blood in this situation, and how are we to determine what is the exact part of the cranium which should be perforated by the trephine? I must here refer to an observation which has been already made. Blood is seldom poured out in any considerable quantity between the dura mater and the bone, except in consequence of a laceration of the middle meningeal artery, or one of its principal branches, and it is very rare for this accident to occur except as a consequence of fracture. If therefore we find the patient lying in a state of stupor, and on examining the head we discover a fracture with or without depression, extending in the direction of the middle meningeal artery, although the existence of an extravasation on the surface of the dura mater is not thereby reduced to an absolute certainty, it is rendered highly probable, and the surgeon under these circumstances would neglect his duty if he omitted to apply the trephine. If it happens that no extravasation is discovered, the operation does not leave the patient in a worse condition than he was in before: but if there be an extravasation, although it does not place him in a state of absolute security, it relieves the present symptoms, and gives him a chance of recovery which he would not have had otherwise.

Where no fracture is discoverable, yet if there is other evidence of the injury having fallen on that part of the cranium in which the middle meningeal artery is situated, the use of the trephine may be resorted to on speculation, rather than that the patient should be left to die without an attempt being made for his preservation. I cannot indeed adduce any particular experience of my own in favour of what is here recommended; but I conceive that the instances which have been recorded, in which the middle meningeal artery has been ruptured without any fracture of the bone, and the known fact that there is sometimes a fracture of the inner table of the skull, while there is none of the outer table, sufficiently justify such an experiment in desperate cases, or even in those in which there is much danger. Our judgment may be assisted on those occasions by attending to the rule laid down by Mr. Abernethy: "If there be so much blood on the dura mater as materially to derange the functions of the brain, the bone to a certain extent will no longer receive blood from within; and by the operation performed for its exposure, the pericranium must have been separated from its outside. I believe that a bone so circumstanced will not be found to bleed, and I am certain that it cannot bleed with the same freedom and celerity as it does when the dura mater remains connected with it."*

In applying the trephine on account of a fracture with depression, the removal of a small portion of bone is generally sufficient; and there is indeed no sufficient reason for removing any considerable portion of the cranium. But in resorting to the application of the trephine, on account of an extravasation of blood on the surface of the dura mater, our practice should be different. The bone should be removed extensively, so as to expose at any rate a large portion of the surface of the dura mater, in which the extravasation has taken place. The necessity of attending to this rule, was impressed on my mind by a case which came under my care in the hospital, in the year 1814. A man was admitted with a fracture of the parietal bone, and a large extravasation of blood, between the cranium and the dura mater. I removed two triangular pieces of bone with a straight saw, and a large quantity of blood, partly fluid, partly coagulated, escaped through the opening that was made. The symptoms under which the patient laboured, were immediately relieved, and for several days he appeared to be going on favourably. But suppuration ultimately took place on the surface of the dura mater, wherever the extravasation had separated it from the bone. The opening made by the saw being in a great measure occupied by granulations from the dura mater, afforded no opportunity for the free escape of the pus

* Abernethy on Injuries of the Head. Edit. 1797. Pp. 33, 34.

which was formed in the neighbourhood, in consequence of which the abscess burrowed between the dura mater and the bone, separating them from each other, much farther than they had been separated originally. As soon as I had discovered what was taking place, I removed another portion of bone with the trephine; but the mischief had now become so extensive that the operation gave scarcely temporary relief, and the patient died. Reflecting on the case afterwards, I could not but acknowledge that if I had removed a larger portion of the bone in the first instance, so as to expose the extravasated blood more completely, the pus which was afterwards secreted could have been freely discharged, and the life of the patient would in all probability have been preserved.

But the most common cause of pressure on the brain is an extravasation of blood within the cavity of the dura mater. Here if there be any large collection of blood in one mass, it is generally in the basis of the cranium; sometimes in the substance of the brain, at other times in the cells between the tunica arachnoides and pia mater. In either of these cases it is beyond the reach of an operation. There may indeed be a large extravasation of blood on the superior surface of the cerebrum immediately beneath the dura mater: but if such an extravasation does exist, in what manner are we to become informed of its existence? We may regard it as a general rule, that an operation is not applicable to cases of compression of the brain from internal extravasation. But there are few general rules in surgery, to which some exceptions may not be made. Let us suppose a case in which a considerable portion of bone has been already removed; in which the dura mater is seen exposed, of a blue colour, lifted up by a collection of blood beneath it, and bulging as it were into the aperture, which has been made in the cranium. Are we not justified in puncturing the dura mater for the purpose of allowing the extravasation to escape? Every thing that we see of wounds of the dura mater tends to prove the very great danger of this kind of injury. The dura mater should never be wantonly punctured; but we cannot doubt that, in what may be regarded as desperate cases, it must be right to give the patient the chance, small as it may be, which the division of the dura mater affords him. The combination of circumstances which would lead to such an operation must be very rare, but it may occur nevertheless, and the surgeon should be prepared to meet it. The late Mr. Chevalier was called to a child a year and a half old, who had received a severe blow on the head. The child lay in a of state insensibility, and was affected with convulsions. There was no wound of the scalp, but on an attentive examination of the head the fontanel appeared to be somewhat elevated. Mr. Chevalier was led therefore to make a crucial incision of the scalp, by dissecting up the corners of which he exposed the fontanel. He then made an angular incision of the right side of the fontanel, and raised the membrane forming it so as to expose the surface of the dura mater, beneath which the purple colour of extravasated blood was plainly to be seen. A puncture being made carefully with a lancet, the blood issued at first with considerable force, spouting to the distance of a foot. Three or four ounces of blood escaped; the symptoms were immediately relieved, and the child recovered without any further unfavourable symptoms.*

The following case, which is still more remarkable, was communicated to me by Mr. Ogle of Great Russell Street, in whose practice it occurred some years ago.

A woman, who kept a cellar in Monmouth Street for the sale of second-hand linen, &c. fell from the street, head foremost, to the bottom of the cellar. When taken up she was in a state of total insensibility. Mr. Ogle being immediately sent for found her lying as if in a fit of apoplexy. He ordered her head to be shaved, and, on examining it afterwards, discovered no wound of the scalp, but observed that she flinched very much when pressure was made on one spot near the anterior and superior angle of one of the parietal bones. Having made an incision of the scalp at this part, he could perceive no appearance of fracture. Nevertheless as the woman was manifestly in imminent danger, he thought it expedient to remove a portion of the bone with the trephine. Immediately on the bone being removed, the dura mater of a dark colour rose into the opening nearly as high as the external surface of the cranium. Convinced from its appearance, and from the feeling of tension communicated to the fingers, that a fluid was interposed between it and the brain, and that that fluid was blood, Mr. Ogle ventured to puncture the dura mater with the point of a lancet. The puncture was instantly followed by a stream or jet of blood, which spirted out to the height of some feet. Immediately on the blood being discharged, the woman, who till that moment had continued totally insensible, opened her eyes. After looking about her, apparently amazed, she exclaimed, "What is the matter? what are you doing with me?" and was able to give a clear account of the manner in which the accident had occurred. From this time she recovered without any untoward symptoms. It was impossible to ascertain the precise quantity of blood which escaped through the opening of the dura mater, but Mr. Ogle supposes it to have been about three quarters of an ounce. But cases such as these are to be regarded as out of the common course of events. The ordinary cases of extravasation within the dura mater from injury are to be treated as we treat cases of apoplexy, or of paralytic seizure, in consequence of a blood-vessel within the head being ruptured from disease: on the same principle as that on which we treat other cases of internal hæmorrhage.

* Medical and Physical Journal, Vol. VIII. p. 505.

Take blood from the arm so as to reduce the force of the heart's action. Repeat this, or take blood by cupping, as soon as the pulse has recovered from the effect of the former blood-letting: administer active saline purgatives; let the head be shaved and bathed with a cold lotion, being kept at the same time in an elevated position; and although such a plan of treatment will not effect the cure of a patient who lies with stertorous breathing in a state of perfect stupor, many will recover under it, in whom the symptoms of pressure have been very urgent. In some instances a slight improvement is perceptible from day to day, until at the end of two or three weeks the patient seems to be restored to his natural condition. In other instances his recovery is less complete, and a partial loss of nervous power may remain for many months; or such a memorial of the accident as, a dilated pupil, a benumbed hand, or a paralytic limb, may exist for a much longer period, for years, or even during the remainder of the patient's life.

The foregoing observations are intended to relate to those cases in which pressure operates on the brain in such a manner as considerably to impair its functions. There are many other cases in which there is reason to believe that there is extravasation of blood within the cranium, although not in sufficient quantity to produce any formidable symptoms. I have already observed that it is sometimes difficult to distinguish such cases from those of concussion of the brain; and it is therefore fortunate that, even where the distinction is plain, it leads to no difference of treatment.

It is also not uncommon for a fracture of the cranium to exist, with even a considerable depression of bone, and for the patient to suffer from it only in a very small degree, or to have no symptoms at all. Mr. Abernethy has published an account of several cases of this description, in which there were not only no symptoms at the time, but none at any subsequent period, although no attempt was ever made to restore the depressed bone to its natural situation, and I might add to the catalogue many similar cases which have fallen under my own observation; but the fact is now well known to every practical surgeon; and in doing so, I should unnecessarily occupy the time and attention of the Society. Here the condition of the patient immediately after the accident does not indicate the necessity of an immediate operation; and a very interesting and important question arises as to the course which the surgeon should pursue, and whether he should, or should not, under these circumstances, resort to an operation for the purpose of elevating or removing the depression?

The removal of a part of the cranium is not to be viewed as a trifling matter, or as an operation which we are warranted in performing without a very sufficient reason. 1st. The process, by which the aperture in the cranium is filled up with new bone, requires many years for its completion, even where the aperture is small; and where it is large, that process is never completed at all. The deficiency of the cranium must render the patient much more liable to suffer from accidental injury than he would have been if the cranium had been perfect. The cicatrix must be more easily penetrated by a cutting instrument, and more likely to give way under the force of a severe contusion than the bone itself; and in the second volume of the Edinburgh Medical Essays, a case is recorded in which, during a violent fit of the hooping-cough, such a cicatrix was lacerated, the dura mater torn, and the brain made to protrude through the wound, the patient dying with paralysis of the limbs five days afterwards. 2dly. Without referring to those remote consequences, or to cases in which it has been carelessly or improperly performed, the operation of the trephine is not to be regarded as one altogether free from danger. I saw a case in which a surgeon was induced to apply the trephine, although, as the event proved, there was no sufficient reason for so doing. The dura mater, at the time of the operation, was found adhering to the bone, and in a healthy state. Nevertheless, when the patient died some time afterwards, the body was examined, the external layer of the circular portion of the dura mater which had been exposed in consequence of the trephine being employed, was found in a state of slough, and it was a matter of doubt whether the sloughing did or did not extend through the whole thickness of the membrane. In another case, which occurred in St. George's Hospital, Mr. Gunning was induced to apply the trephine, in consequence of a suspicion that suppuration had taken place between the bone and the dura mater. The suspicion proved to be ill-founded: the dura mater was in a perfectly natural state, and there was bleeding from the small vessels on its surface after the renewal of the bone. The patient died afterwards in consequence of inflammation of the brain and pia mater. On dissection, besides the usual appearances produced by such inflammation, it was found that the circular portion of the dura mater which had been exposed in the operation was in a state of slough, the slough extending through its whole substance. Everywhere else the dura mater was in a natural state. It is reasonable to conclude that the sloughing of the dura mater in these cases was the consequence of its being deprived of its natural protection, and of the supply of blood which it receives through the vessels of the bone.

Now if the patient, whose case was mentioned last, had survived some time longer, what would have happened? The slough of the dura mater would have separated, and the brain losing the support which it derives from this firm membrane, and having its vessels loaded with blood, would, in all probability, have become protruded in the form of what is denominated a *hernia cerebri*. Such a protrusion would not indeed aggravate the dan-

ger of the case, where suppuration had already taken place within, but it might make the difference of life or death to the patient where the inflammation had not begun to terminate in this manner.

That the removal of a portion of the cranium may in itself be sufficient to make the patient liable to this formidable and dangerous disease of *hernia cerebri*, would appear sufficiently probable without any particular experience on the subject; and for evidence that this is actually the case, we need not go further than the Transactions of this Society. I allude to the very important paper by Mr. Stanley, published in the eighth volume of this work. In every one of four cases, which are here recorded, in which a portion of the bone of the cranium had been removed by the trephine or straight saw, the dura mater was found not to have suffered from the injury, yet a *hernia cerebri* presented itself some days afterwards. In one case it is distinctly stated that the dura mater was in a state of slough at the time of the protrusion beginning to take place; but it does not appear whether in the other cases it gave way in consequence of sloughing or ulceration.

Taking all these facts into consideration, we cannot refuse our assent to the proposition that the perforation of the skull, and the removal of a part of it, is attended with a certain degree of danger, and the evidence hitherto adduced is in favour of the opinion, that "it is most prudent to abstain from the use of the trephine, where there is a fracture with depression of the cranium producing at the time no unfavourable symptoms."

But much may be said on the other side of the question; and at any rate there are other points to be considered before we can arrive at a positive conclusion on the subject.

1st. Although in some cases sloughing of the dura mater and *hernia cerebri* may follow the operation of the trephine, there are many other cases in which this never happens, the dura mater granulating, and the wound cicatrizing favourably.

2dly. Notwithstanding that a depression of the cranium is allowed to remain in many instances without its being productive of any bad consequences, there are numerous examples of such an injury being followed by extensive mischief. Suppuration takes place on the surface of the dura mater, an abscess is formed between that membrane and the bone, and ultimately (as I shall endeavour to explain on a future occasion,) if the abscess has no opportunity of discharging itself externally, the inflammation extends to the parts below, and there is suppuration of the tunica arachnoides and pia mater, leading inevitably to the patient's destruction.

3dly. Where a depression of the cranium is allowed to remain, it sometimes happens that symptoms arise after a considerable lapse of time, which may even endanger the life of the patient, and which are to be attributed to the continuance of the depression, although it had occasioned no inconvenience in the first instance. I saw a well-marked and very instructive case of this kind several years ago under the care of Sir Everard Home, of which Sir Everard has published some account in the Philosophical Transactions for the year 1814. A gentleman received a blow on his head in consequence of having fallen from his horse, which occasioned a fracture and depression of one parietal bone. The depression was two inches and a quarter in its longest, and an inch and a half in its shortest diameter, and in one part nearly three quarters of an inch below the natural level. At the end of six weeks the early symptoms had subsided, and the patient was considered well. As soon however as he returned to his usual occupations, various nervous symptoms began to show themselves, which manifestly depended on the continued pressure on the brain.—These symptoms, instead of diminishing, increased in severity, and on some occasions were such as to occasion serious alarm; in consequence of which, at the expiration of three years from the time of the accident, Sir Everard was induced to remove nearly the whole of the depressed bone with the trephine. The wound cicatrized readily. The symptoms which existed before the operation were immediately relieved, and, as I have been informed, never recurred.

In this case the fracture and depression were very extensive, and probably these ultimate ill consequences, or secondary effects of the injury, may be avoided, if we consider it as a general rule, that an extensive or deep depression should lead to the application of the trephine, although the same necessity does not exist where the depression is small.

This rule however affords us no assistance with respect to the greater danger arising from the chance of suppuration between the bone and the dura mater; this being as likely to occur where the depression is small as where it is large.

Sir Astley Cooper has stated in his Lectures on Surgery* that there is a great difference as to the danger of inflammation and suppuration of the membranes of the brain, between those cases in which the fracture and depression is complicated with a wound of the scalp, and those in which the soft parts are uninjured; such mischief being much more liable to occur in cases of the first kind than in those of the second: and on these grounds he recommends that, where this complication exists, we should not hesitate to apply the trephine; and on the other hand, that, where it does not exist, we should carefully abstain from adding to the injury, by dividing the scalp and exposing the fracture. But many persons undoubtedly have recovered in whom there was at the same time a wound of the scalp, and a fracture and depression of the cranium, although no operation was resorted to. The cases to which I have before alluded as pub-

* The Lectures of Sir Astley Cooper, Bart. by F. Tyrrell, &c. Vol. I.

lished by Mr. Abernethy, are all examples of this fact; and I recollect other similar cases which have fallen under my own observation. I have conversed also with several other surgeons whose experience on the subject has corresponded with my own, and all these circumstances led me in the first instance to doubt the accuracy of Sir Astley Cooper's conclusion.*

The question however is not to be decided merely on these premises. Many persons may do well without an operation, who suffer from what Sir Astley Cooper denominates a compound fracture of the cranium, and yet it may remain to be determined what is the probability of suppuration taking place in these cases, as compared with those in which the scalp escapes uninjured?

For many years I have preserved notes of a large proportion of the cases of injury of the head, which it has fallen to my lot to witness. Among them, of course, are many in which there was fracture, with or without depression, followed by suppuration between the dura mater and the bone. On referring to these for further evidence on this interesting subject, I find that the cases in which suppuration takes place where the scalp is entire have been comparatively rare; bearing a very small proportion indeed to those cases in which suppuration has followed a fracture complicated with a wound of the scalp. Such is the result of my own experience, during a considerable period of time, and which I am enabled to give not merely from a general recollection of what I have seen, but on the authority of written notes, made at the bedside of the patients, and for the most part before the question which they illustrate had ever presented itself to my mind.

Taking all these facts into consideration, and endeavouring to give its proper value to what may be urged on either side of the question, I cannot but acknowledge, whatever may have been my first impression on the subject, that it appears to me at this moment that the views of Sir Astley Cooper are well-founded; and that, in those cases in which depression of bone exists without any symptoms, or with only trifling symptoms arising from it, the surgeon can follow no better general rule than this: if the depression be exposed in consequence of a wound of the scalp, let him apply the trephine, and elevate the depression: but if there is a depression without a wound of the scalp in consequence of the accident, let him not make such a wound by an operation. An exception may perhaps be properly made with respect to very extensive depressions of the cranium, which it may be prudent to expose and elevate at all events, not because there is a greater danger of suppuration from these than from smaller injuries, but on account of the ultimate ill consequences which the patient may experience if the brain be left permanently subjected to a very considerble pressure.

I have only two further observations to offer before I leave this part of the inquiry.

The first is, that even where the fracture and depression of bone is complicated with a wound of the scalp, there is not, in all cases, the same absolute necessity for the application of the trephine. The bone may be depressed in such a manner as to allow the escape of the pus which is formed on the surface of the dura mater, although the depression is not elevated; or its position may be such as that the abscess can find no external opening. The danger in the first case must be infinitely greater than that in the second. A boy was admitted into St. George's Hospital who had received a severe blow on the head. The scalp was wounded, and there was a fracture and depression of bone: but as the depressed bone was not of a large size, as it was not much below its natural level, and as it produced no symptoms of importance, I did not apply the trephine. Eight or nine days after the accident, the boy complaining of pain in the head, the pulse having become fre-

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* The following statement was furnished to me by my friend and colleague, Mr. Rose, from notes which he made while surgeon to the Coldstream Regiment of Guards during the Peninsular war:—

"In the battle of Talavera de la Reyna, which was fought on the 27th and 28th of July 1809, the brigade of guards lost about 600 men in killed and wounded. Amongst the latter were a considerable number of cases of wounds in the head. There were a great many cases of fracture of the bones of the cranium with and without depression, and from the cause which produced them, these were, of course, in every instance complicated with wounds of the scalp.

"On the third of August, in consequence of some military movements, the town of Talavera, in which the hospital had been formed, became exposed, and an order was given for the wounded who could march, to leave it. This was so speedily obeyed that no time was afforded to make any selection. The worst cases necessarily remained, but among those who undertook the march there were twelve or fourteen with wounds in the head, accompanied with injuries of the bone, at least four or five of whom had both tables of the skull fractured, and two of them, along with fracture of the os frontis, had each the globe of one eye totally destroyed. In none of them had the trephine been applied, nor had any attempt been made to remove splinters of bone. After leaving Talavera, they were exposed to a burning sun, and to very severe fatigue. Every evening, after the day's march, Mr. Rose collected the wounded round him, examined and washed their wounds, dressing with care those that particularly required it. Cold water was the principal application employed. The retreat occupied sixteen days, in spite of which, and with no other treatment than that which has been described, every one of those who were wounded in the head recovered."

quent, and there being an expression of anxiety in the countenance, I divided the scalp beyond the wound which already existed, so as to expose the fracture more completely. I now discovered that suppuration had taken place beneath the bone, but the edge of the depressed bone was so much below the level of the bone in the neighbourhood that there was a very free opening for the escape of the pus, which was distinctly seen at the bottom of the wound, rising and falling as the pulsations of the brain were communicated to it. No further operation was performed. The symptoms were relieved by the more free division of the scalp; the wound healed, and the patient left the hospital quite recovered about five weeks after his admission.

The last observation relates to a circumstance, the possible occurrence of which adds to my unwillingness to divide the scalp in cases of fracture and depression of the cranium, where it has not been already divided with violence inflicted on it at the time of the accident. I have seen two cases in which the scalp remained entire, but in which the bone was fractured and depressed, and the dura mater lacerated, and the brain itself wounded by the edges of the fracture. Such a complication may be sufficiently dangerous under any treatment; but if we are to judge from the analogy of what occurs not only in cases of simple and compound fractures of the extremities, but of other mechanical injuries, we must suppose that the danger would be much aggravated by the addition of a wound of the scalp. Suppuration of the brain and its membranes, to a greater or less extent, must necessarily ensue, if they are exposed under the circumstances which have been described, and it seems not improbable that such mischief may be avoided if the scalp be allowed to remain entire for their covering and protection. Of course this remark applies only to the conduct of the surgeon in the first instance. The treatment to be pursued, if, at a later period, suppuration should be actually established, is not under our present consideration.

Sect. 9.—*Treatment of Contusions and Wounds of the Scalp.*

As the treatment to be employed in cases of concussion and compression of the brain involves questions of peculiar interest, which demand the earliest attention of the surgeon in the greater number of instances of injury of the head, I have thought these subjects not undeserving of our first consideration. It remains for us to determine the course which is to be pursued in other cases, of which the principal are, those of wounds and contusions of the scalp; and of fracture, unattended with depression, where there is no reason to believe that there is extravasation of blood beneath the bone; and those of wounds of the brain or its membranes.

Extravasation of blood in the cellular texture of the scalp seems to require for the most part no particular attention. Here, as elsewhere, the swelling made by the extravasation gradually becomes less prominent, and more diffused, and no great length of time elapses before it disappears altogether. I was consulted concerning the case of a young gentleman in whom there was an effusion of blood under the scalp, extending from the superciliary ridges to the nape of the neck, and from ear to ear. When I saw the patient the blood appeared to be still in a fluid state, or at any rate not completely coagulated; and it had been poured out in such quantity that the cranium itself was not in any part perceptible to the touch: nevertheless, in the course of a few weeks, with no other application than that of a cold lotion, the whole tumour disappeared.

It is evident that, whatever was the vessel ruptured in this instance, it must have continued to bleed for a considerable time before so large an extravasation could have taken place. In another case in which a vessel under the scalp was bleeding in the same gradual manner, and threatening to produce similar results, I was enabled to ascertain the point at which the extravasation began, and by making pressure in this situation to stop its further progress. The patient was a child who had received a blow on one temple, I believe, from the corner of a table. Soon afterwards the nurse observed a swelli g in the part which had been struck, which however attracted but little attention at the time. On the following day the swelling had increased, and the parents brought the child to London, a journey of several miles. During the journey, the swelling became still larger, and when I was consulted soon after their arrival in London, it occupied the whole temple. I directed the child to be kept quiet, and the head to be bathed with a cold lotion. Next day, however, the swelling had extended over a considerable part of the head adjoining the temple, presenting an appearance exactly similar to that which was observed in the case last mentioned. I now inquired of the nurse, more particularly than I had done before, what was the exact spot at which the head had been struck, and in which the swelling was first discovered; and having ascertained this, I applied a graduated compress and bandage, such as is used after bleeding in the temporal artery: and from this time there was no further increase of the swelling.

Punctured and incised wounds of the scalp require (in the first instance at least) no peculiar treatment. Nothing that has occurred in my own experience would lead me to believe that there is any reason why adhesive plaster should not be employed to approximate the edges of a wound of the scalp, as well as those of a wound elsewhere. Erysipelas not uncommonly follows a wound of the scalp, but it seems to me to occur equally, whether the wound is dressed with adhesive plaster or in any other manner.

When a portion of the scalp is separated in the manner of a flap, so as to expose the tendon of the occipito-frontalis muscle, or the pericranium, if it be carefully and neatly re-

placed, it will often become united by the first intention to the parts from which it has been separated. In many cases however there will be no adhesion, as where some time has elapsed before the wound has been dressed; or there has been considerable contusion; or the surface of the wound has been smeared with dirt, or other extraneous substance. In other cases there will be partial adhesions, some parts of the wound becoming united while there is suppuration elsewhere; and (as I shall have occasion to observe hereafter) this state of things requires much attention on the part of the surgeon, lest the formation of abscesses in certain places should do injury to the pericranium and bone, and destroy the adhesions in the neighbourhood.

In those cases also, in which the pericranium is separated from the bone, it is for the most part right to replace the scalp, with the torn surfaces in contact, and to allow them to have the chance of becoming united, whatever that chance may be. Such union will not unfrequently take place even in the adult, where the bone is not exposed to a great extent, and the parts are nicely adjusted to each other; but there is much more reason to expect it in the young person, on account of the greater vascularity of the harder textures before the period of growth is concluded.

Sect. 10.—*Treatment of Fractures of the Cranium unattended with Depression.*

It seems to be the general opinion of modern surgeons that a fracture of the cranium, where there is no depression, and no evidence of any considerable extravasation between the dura mater and the bone, requires nothing beyond the strict antiphlogistic treatment, which ought to be resorted to in all cases of injury of the head. The fractured surfaces being here in contact are under circumstances the most favourable to the process of union, and the removal of a portion of the bone with the trephine must be regarded as a considerable, and as far as the fracture itself is concerned, a wanton addition to the mischief already inflicted, which, instead of expediting, cannot fail materially to retard the patient's ultimate recovery.

The application of the trephine, under these circumstances, has nevertheless been recommended by Mr. Pott; and I should be guilty of a serious omission if I were to pass over in silence a question of such importance, and relating to a point of practice which has received the sanction of such high surgical authority.

In the perusal of Mr. Pott's treatise on Injuries of the Head, we cannot but feel some degree of astonishment that that eminent surgeon should have resorted to an operation with so little hesitation in a number of cases, in which the existing symptoms were of trifling importance, and in which there was no evidence of immediate danger. It does not appear however that Mr. Pott, on these occasions, acted merely under the influence of his early prejudices, or of the example of those who had gone before him; and although not formally stated in his writings, the following argument may be deduced from them in favour of the practice which he recommended and adopted.

1st. The blow which occasions a fracture of the cranium, is likely to do such further injury to the vessels of the dura mater as may lead to inflammation and suppuration of the external surface of that membrane, and the formation of an abscess between it and the bone.

2dly. If such an abscess be formed without a free external opening, the case must terminate fatally.

3dly. If immediately after the accident a portion of bone be removed by the trephine, the pus formed afterwards on the surface of the dura mater is enabled to escape, and the danger arising from its confinement beneath the bone is avoided.

But it may be urged in opposition to this doctrine, 1st, That Mr. Pott seems, on the one hand, to have greatly over-estimated the danger of suppuration between the bone and the dura mater in cases of simple fissure of the cranium; and that such mischief will be avoided in the very great majority of cases, provided that, from the moment of the accident, the patient be kept in a state of perfect repose, on a spare diet, with the head cool, blood being taken occasionally from the arm, and these remedies being combined with the use of saline purgatives.*

2dly. That he seems on the other hand to have under-estimated the evils which may arise from the removal of a portion of the cranium, to which in fact no allusion is made in any part of his writings. On this subject it is needless to repeat the observations which I have made in a former part of this paper.

3dly. That even if suppuration should take place between the bone and the dura mater, a watchful surgeon may generally detect the circumstance before pus has been formed to any great extent, and that the patient has still the chance of being preserved by the timely application of the trephine.

We can scarcely hesitate to admit that the reasons for abstaining from the use of the trephine under the circumstances which have been described, are more conclusive than those which may be urged in favour of a more active treatment; it being at the same time borne in mind that cases of fracture of the cranium, even without depression, are always to be re-

* Whoever reads Mr. Pott's observations on this subject, and compares them with what is now seen in hospital practice, will, if I am not mistaken, find good reason to believe that suppuration between the dura mater and the bone in consequence of a fracture, is less common at the present period than it was, when Mr. Pott wrote; a difference which may fairly be attributed to the more strict antiphlogistic treatment, which modern surgeons do not fail to adopt in all cases of injury of the head, whether the early symptoms be or be not of a dangerous description.

garded with a jealous eye, especially where the scalp is wounded and the pericranium separated from the bone, it being in these cases especially that danger exists of the formation of matter between the dura mater and the bone.

Sect. 11.—*Treatment of Wounds of the Brain and its Membranes.*

Although the condition of the patient who labours under a wound of the brain, or dura mater, is essentially different from that of one in whom no such wound exists, the general treatment required in these two orders of cases is nearly similar; and bleeding, purgatives, low diet, and a state of perfect repose, form an important part of the remedies to be employed in cases of wounds, as well as in those of concussion and compression of the brain.

The object of the local treatment, where there is a wound of the brain or its membranes, is not so much to relieve the existing symptoms as to prevent future ill consequences, the principal of which are (as I shall show hereafter,) 1st, inflammation extending from the wound over the membranes of the brain, and producing an effusion of serum and pus; 2dly, inflammation, suppuration, sloughing, and dissolution of the substance of the brain; 3dly, protrusion of the brain, in the form of what is commonly denominated a *hernia cerebri*.

A judicious surgeon will always bear in mind, that, especially on these occasions, the first rule of his art is not to add to the mischief already done. If splinters of bone have penetrated into the brain, and can be removed with perfect facility, and without the smallest additional disturbance to the injured organ, such removal cannot be improper, and may probably be useful. Many persons however have recovered, in whom an opposite practice has been pursued. I saw a gentleman in whom detached fragments of bone remained imbedded in the brain, many months after he had received a wound in the head from a pistol bullet, and who suffered scarcely at all from the injury. Do not such cases justify us in leaving splinters of bone untouched, where there is any kind of obstacle to their easy extraction? Are they not even sufficient to show that any other mode of proceeding would be improper, and that it is better to leave the patient to take his chance with the splinters lodged in the brain, than to commit the smallest additional violence in an endeavour to remove them?

A similar observation may be made respecting depressions of bone when complicated with wound of the brain. If the edge of the depressed bone be imbedded in the substance of the brain, it may be proper to restore it to its natural level, provided that this can be readily accomplished with the forceps or elevator. But individuals have recovered, in whom a depression of bone has been allowed, under these circumstances, to remain without being elevated; and it cannot be advisable to risk this chance of recovery, whatever it may be, if the elevation requires the application of such a degree of force as is likely to cause the most trifling additional injury to the wounded brain. I have myself been led to doubt the expediency of applying the trephine in those cases in which there were no circumstances making the operation absolutely necessary. The motion of the saw must occasion more or less jar to the tender substance of the brain; and this, which may be of little consequence where the brain and its membranes are entire, may make a serious difference as to the degree of danger, where these parts are already lacerated and contused. There is, moreover, the same objection here as in other instances, to the removal of any considerable portion of the parietes of the cranium, namely, the liability which it occasions to the formation of a *hernia cerebri*.

The lodgement of a musket-ball, or other foreign body, in the substance of the brain, is undoubtedly a very serious occurrence, and one attended with the greatest danger to the patient. If the foreign body be of such figure and dimensions, and so situated, that while one extremity of it is inclosed within the cavity of the cranium, the other extremity projects externally, it may of course be extracted, and, probably, ought to be extracted at all risks. But with respect to a musket-ball or pistol-bullet lodged in the brain, it may be observed, first, that it rarely happens that it can be discovered and extracted even by the lightest and most practised hand, without such a degree of violence as must be in itself sufficient to produce a train of evils, which in all probability would terminate in death: and, secondly, that there are numerous instances of persons who have recovered, although the ball was allowed to remain in the brain; some of whom have suffered no more than they would have suffered from its being lodged in a less important part of the body. Taking all these things into consideration, ought we not to regard it as the general rule, that the extraction of a ball should not be attempted; an exception to the rule being made only in those cases, in which, from its more superficial situation and other circumstances, the extraction can be easily accomplished without the employment of force, and without adding in any degree to the mischief already done?

On the whole (according to the view which I am led to take of the subject,) there seems to be in the very great majority of cases of wounded brain, more wisdom in resorting to negative, than to active local treatment. At any rate, as the restorative powers of the animal system are on all occasions the principal agents in the reparation of mechanical injuries, we cannot be wrong wherever there is a reason for doubt as to what should or should not be done, in leaving nature to take her own course, in trusting to her efforts rather than to human science and art.

My own experience, as far as it goes, is in favour of what is here recommended. I do not mean, however, to assert that what I have seen of cases of wounded brain is in itself suf-

ficient to justify me in forming these conclusions, unaided by a general knowledge of disease, and by arguments derived from analogy. In fact, the cases of wounds of the brain, which occur in the routine of a civil hospital, are so few in number compared with those of other injuries of the head; they exhibit such numerous and various complications; and the proportion of recoveries from such wounds, whatever system is pursued, is so small (especially among adult patients,) that it would be bold of any surgeon, engaged in the ordinary duties of his profession, to declare that he had been able to make a comparison of the different modes of treatment on such an extended scale, as would enable him to lay down rules of conduct founded wholly on his own practice and experience. The opportunities of military and naval surgeons must be, at certain periods, more considerable, but the circumstances under which they occur are very unfavourable to that minute observation and accurate judgment, which would be necessary to enable them to derive from their opportunities, all the advantages, which they might otherwise afford. Where the experience of individuals fails, we are called on to look for other sources of information. I have referred to all the cases of wounded brain recorded in the works quoted below,* and the general results which they exhibit will be found not uninteresting, if viewed in their relation to this point of surgical practice. These cases are thirty-eight in number, of which twenty-six terminated favourably, and twelve unfavourably. This, of course, affords no information as to the actual rate of mortality in cases of this description, the fatal cases being for the most part regarded as too much a matter of course to be worthy of publication, while a very different opinion is entertained respecting the cases of recovery. But the following facts afford some useful information as to the circumstances under which recovery takes place.

In nine cases of wounded brain in which the bone was fractured, but not depressed, no operation whatever was performed. In two of them the patients died; in the remaining seven they recovered.

In fifteen cases no operation was performed, beyond that of removing some splinters of bone with the forceps. In five of these cases the patients died, while in ten the patients recovered.

In four cases the wound of the brain was complicated not only with fracture, but with depression of bone. In one of them in which the depressed bone was allowed to remain without being elevated, the patient recovered. In the three remaining cases the depression was elevated with the assistance of the trephine; and one of these patients recovered, and two of them died.

In ten cases a musket-ball was lodged in the brain. In two of them the ball was extracted, and one patient recovered, while the other died. In the remaining eight cases the ball was allowed to remain, no attempt being made for its extraction, and two of these patients died, while six of them recovered. Of these last, however, one died several weeks afterwards of inflammation of the brain induced by intemperance in drinking, and another after having been sufficiently well to resume his duties as a soldier, died in the course of the following year, of what was regarded as a *coup de soleil*.

It appears then that in fourteen out of twenty-six patients who recovered, no operation whatever was resorted to, and that in ten of the remaining twelve, there was no operation beyond that of removing splinters of bone with the forceps. Of those in whom a ball was extracted from the brain one died, and one recovered; and of those in whom the ball was not extracted two died, and six recovered. It is needless to add, that the conclusions to be deduced from these statements illustrate and confirm the observations which have been already made as to the principles which should direct the surgeon in his treatment of these formidable injuries.*

There is one circumstance connected with this subject, which is too important to be passed over in silence, and which may very properly be mentioned in this place, as it must very materially influence us in the opinion which we give, at the time of the accident, as to the probability of the patient's recovery. I have not been able to discover, among all the works which I have consulted, a single

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* Mémoires de l'Académie Royale de Chirurgie.—Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge.—Duncan's Medical Commentaries.—Duncan's Annals of Medicine.—Edinburgh Medical Journal.—Medico-Chirurgical Transactions, Vol. I. to Vol. XII. inclusive.—Le Dran's Observations in Surgery.—Hennen's Military Surgery.—Collection d'Observations Cliniques par M. A. Petit.

* Since these calculations were made, a very interesting case has been published by Dr. Rogers in the thirteenth volume of the Medico-Chirurgical Transactions,† in which the breech-pin of a gun was lodged in the anterior lobes of the cerebrum, and extracted at the end of twenty-eight days, the patient afterwards recovering. Here the foreign body remained in the substance of the brain until inflammation had been going on for an entire month, so that the brain and its membranes must have become extensively agglutinated and consolidated around it. The question as to the extraction of a foreign body after such a lapse of time, and under such circumstances, belongs more properly to another part of these inquiries; my observation at present being intended to be confined (as nearly as that can be done) to the treatment to be employed immediately or soon after the occurrence of the injury.

† Vide Journal of Foreign Medicine, Vol. I. page 213.

instance of recovery from a wound of the posterior lobes of the cerebrum, of the cerebellum, or medulla oblongata; and in the great majority of cases in which a cure has taken place the injury has been confined to the frontal bone, and that part of the brain which is covered and defended by it.

SECT. 12.—*On the Treatment of some other Cases which are not included under the foregoing heads.*

In those cases in which a particular class of sensations is destroyed or impaired,—as where deafness, or a loss of smell or taste follows an injury of the head,—I am not aware that advantage is to be expected from any particular mode of treatment, or that the use of any remedies is indicated beyond those which are resorted to in ordinary cases of concussion of the brain. The patient generally recovers the sensations of which the accident had deprived him in the course of one or two years; but his cure is to be attributed not so much to the skill of the surgeon, as to the restorative powers of his own system. All cases however do not prove equally fortunate in the result. I saw a gentleman in consultation with Dr. Francis Hawkins, who had lost his sense of smell in consequence of a blow on the head many years before, and in whom no improvement in this respect had ever taken place. He was not even sensible of the odour of valerian, although he could distinguish the medicine by the taste.

When a patient is affected with furious and raving delirium, blood should immediately be taken from the arm, and, if possible, in a full stream. I scarcely remember a single case, in which delirium of this kind, occurring soon after a blow on the head, did not yield to a copious blood-letting. The patient may very probably relapse into the same state, as soon as the first effects of the loss of blood have subsided, and it may be necessary to resort to the same means a second or third time, before the relief is permanent.

As there is reason to believe that convulsions and furious delirium occur under nearly parallel circumstances, we may expect that the treatment which is useful in cases of the former description, will also be useful in those of the latter. When convulsions occur soon after the accident, blood-letting is undoubtedly indicated. It is not however always easy, when the patient's limbs are thus moved and agitated, to succeed at once in the operation; and in many cases, the convulsions having been once established, they will continue for a certain period, notwithstanding that a considerable quantity of blood has been taken away, subsiding at last spontaneously. In a few instances (as has been already explained) convulsions occur at the expiration of some days from the time of the injury. At this later period they may exist in combination with inflammatory symptoms, which may require the further use of the lancet. But they may also exist independently of inflammation, being aggravated by any additional abstraction of blood, and subsiding on the patient being allowed to take some more substantial nourishment than that which had been allowed him previously.

A case has been related in a former part of this paper, which will serve to illustrate this last observation.

From the London Medical Gazette.

ESSAYS ON SYPHILIS. By JOHN BACOT, lately Surgeon to the First Regiment of Guards.

[Continued from page 424.]

This, and the following essay, will be principally devoted to an examination of the writings, and a detail of the opinions, of authors of the present day. Nevertheless, it will be my duty not only to detail fairly and impartially the result of their labours, but also, as the occasion presents itself, to make such comments upon their doctrines as they appear to require; and to point out, without reserve, the errors into which it appears to me that some of then have fallen.

I have already mentioned the general state of practice in syphilis at the time Mr. Hunter published his Treatise on that disease: particular points of doctrine were, indeed, the occasional subjects of discussion in different publications and lectures; but practically, no one dreamed of curing the complaint without a course of mercury, still less was it imagined that the symptoms *could* be cured in any other way, although it now is quite certain that on the continent of Europe, and more especially in Germany, the common plan of treatment had undergone a considerable change—that the corrosive sublimate had there become the favourite remedy; but even that medicine was prescribed in very inefficient doses, according to the dogmas of the day, as taught in this country. The Peninsular war, however, opened to the medical officers of the British army new views relative to syphilis, and they lost no time in communicating to the profession the information they had thus acquired. Of these, Mr. Ferguson was the first who published an account of what he had seen in Portugal: his paper is to be found in the fourth volume of the Medico-Chirurgical Transactions. From a perusal of this paper, it is evident that this gentleman considered the conclusions to which he arrived as totally inapplicable to this country, though true as far as they regarded the natives and the climate of Portugal. Mr. Ferguson's opportunities of observing the venereal disease in the Peninsula were very extensive, since he had held the situation of Inspector of Hospitals to the Portuguese army upwards of two years before he wrote his paper, which is dated in May, 1812. It contains some highly interesting paragraphs, which it will be necessary to bear in mind, since they tend in no inconsiderable degree to explain what has hitherto appeared most obscure and difficult of solution in this intricate inquiry. The facts we learn from this paper are principally the following:

—It was customary among the native practitioners in Portugal to cure all primary venereal affections with topical applications only; the native soldiers, as well as those in civil life, were accustomed to perform their duty, and follow their usual avocations, with sores on the penis, not merely such as were of a trivial nature, but such as made Mr. Ferguson shudder to look upon; the only difference in the treatment adopted by the military and civil practitioner in such cases being, that the latter generally combined the decoction of the woods with the local remedies, but in both instances the use of mercury was reserved for those in whom the bones had become affected, when a very small quantity, usually of calomel, was prescribed, together with Dover's powder, warm baths, and other sudorifics. Dreadful examples of mutilation did, indeed, sometimes occur; but these bore no proportion to the number of those who had suffered from the primary symptoms of the disease; and the affections of the bones, when they did occur, were usually slight; thus proving, that in this climate at least, the complaint had become so much mitigated, as to run generally a mild course, until it at length exhausted itself spontaneously.

Very different, however, was the progress of the symptoms in the British army: among the soldiers its ravages were so frightful, that Mr. Ferguson says it is probable that more men had sustained from this cause the most dreadful of all mutilations, during the four years the army had been in Portugal, than the registers of all the hospitals in England could have produced in the last century; so that, not only were the primary sores more intractable to mercury than in England, but also secondary symptoms made their appearance in no small proportion, even whilst the constitution was actually under the influence of mercury.

Such are the principal facts which Mr. Ferguson has detailed. I now come to consider the reasonings he has founded upon those facts. After inferring that syphilis has lost much of its virulence in Portugal, or in other words, has exhausted itself, he remarks that the same change has occurred in the same country with respect to the small-pox, which is permitted to run its natural course unmolested; and so mild has it become, that not one case of fatal termination presented itself to Mr. Ferguson's observation: yet he adds, "I have no doubt that this mild disease, communicated to a tribe of Indians or to a plantation of negroes, or any other class of people, who had never before known the small-pox, would desolate with all the fury of a pestilence wherever it could find victims, and never cease until it had destroyed the whole population." Applying this analogical reasoning to syphilis, he considers the inoculation of the virus of this mitigated form of lues venerea into the constitution of the British soldier, as having produced a disease of more than ordinary violence; and here we cannot fail to observe the effect of early impressions, for Mr. Ferguson remarks, contrary to the direct tenor of the cases he proceeds to detail, that this new organization of disease cannot be combatted by such means as the natives employ, and concludes that mercury affords to the patient the only chance of salvation; yet, strange to say, the detail of a very interesting case teaches us that bleeding, cold lotions, free purging, and the strict antiphlogistic regimen, were the true and efficient means of safety, and not the exhibition of mercury in any shape whatever. The case that calls for this observation is that of an officer, whose penis, four days after a suspicious connexion, became enormously swollen, of a deep red colour, with malignant ugly-looking sores on different parts of the prepuce, and two on the glans penis, which are compared, in appearance, to holes made by a rusty nail in a piece of mahogany or logwood: the general health was also proportionably deranged. The effect of the depletory plan of treatment above-mentioned was magical; but although Mr. Ferguson had no doubt that the violence of the inflammation had superseded the specific contagion, yet, in compliance with old custom and the patient's fears, a mercurial course was afterwards pursued. Another curious circumstance relative to this case must not be forgotten: this officer had been infected by an opera-dancer at Lisbon, who continued for several months afterwards on the stage occasionally infecting others, but without communicating a disease of any peculiar or extraordinary malignancy in any other instance. Mr. Ferguson makes one other observation, which I shall extract, since it is highly deserving of consideration:—"I think it is probable (he says) that, by the resistance we in England have opposed to syphilis and variola, we have retarded their natural decay among us; that we have made both more rare I believe, and that we may finally succeed in extinguishing them I devoutly hope; but whenever we are revisited by either the one or the other, I fear they will not come to us disarmed of their terrors." There are three points in the above narrative which I think ought to be borne in mind, because they are not only of considerable importance in themselves, but because I shall have occasion to revert to them more particularly on a future occasion; they are these—1st, the cure of the officer's ulcers by bleeding, purging, &c.; 2dly, the fact of the same woman communicating a disease of a milder nature to other men; and, 3dly, the conjecture that probably a more severe form of syphilis may at some future time appear amongst us.

Pursuing the course of my history, I have next to mention a very important document, for which we are indebted to Mr. Rose, who, having himself served several years in Portugal, was well qualified to form an estimate of the comparative merits of the two plans of treating syphilis, both Portuguese and English, and who, soon after his return to the Peninsula, adopted the only rational plan—that of putting the question to the test of experiment, discarding all preconceived notions, and looking solely to the natural progress of the dis-

ease when left to itself. The results of these experiments, made in the hospital of the Coldstream Regiment of Guards, during a period of nearly two years, were given to the world in the year 1817. In this publication Mr. Rose announced, that during the above period, he had been enabled to cure *all ulcers* on the parts of generation that had presented themselves, as well as the constitutional symptoms to which they give rise, without the exhibition of mercury. Mr. Roes does not assert that the sores in all these cases were syphilitic; but he tells us, that the battalion in which they occurred consisted of upwards of a thousand men, stationed in London, accustomed to associate with the lowest class of prostitutes, and, therefore, must have afforded (independently of the character of the sores) many undoubted instances of the disease. These, and some other prefatory remarks, are followed by the detail of nearly thirty cases of ulcerations of the genitals, which are divided into three classes: the first includes those not followed by secondary symptoms; the second, those followed by papular eruptions and other symptoms; and, thirdly, of those in which the eruptions differed from the papular form. The only general remark that I shall make respecting the first class is, that the sores were, with exceptions, either attended with much inflammation or sloughing, thus rendering it probable that the rapidity of their progress had superseded the absorption of the poison; a fact of which Mr. Pearson has alluded, in speaking of the efficacy of the cinchona in certain spreading sores on the penis. With respect to the second and third classes, it would seem probable that the occurrence of secondary symptoms was the result of the great length of time that these ulcers had been permitted to run their course, before any plan of cure was sought for by the patients themselves; and this is conformable to the opinion maintained by many medical authorities, that the permanence of the cure, and the security of the constitution, depends much upon the speedy extinction of the virus by mercurial action, where there is nothing in the character of the sore to forbid its use. Mr. Rose's paper concludes with some ingenious reasonings, founded upon the result of this practice; but it does not enter into my views to notice this now: the only conclusion I have to draw from what he has related is, the undoubted fact of every form of primary ulcer on the genitals being curable without mercury; and also the possibility of conquering the constitutional affections that supervene in consequence, without administering a particle of that medicine. During a period of two years, it is to be likewise remembered, that only one or two affections of bones had occurred, in no instance leading to caries. The publication of Mr. Rose's paper made a great impression on the medical public; it excited the curiosity of the profession highly, and stimulated many, who, from their situations as army surgeons, had an opportunity of confirming these experiments by adopting a similar line of conduct, to repeat them. In the several regiments of Guards this plan had been the object of emulation for some time past: at the military hospitals at Chatham and Fort Pitt, as well as at York Hospital, Chelsea, it was likewise resorted to. And in the same volume which contains Mr. Rose's Essay, is to be found a communication on the same subject by Mr. Guthrie. With that gentleman's reasonings I have nothing at present to do; I quote him solely for the purpose of confirming what had been before advanced relative to the cure of *all ulcers* indiscriminately without mercury. His evidence, then, goes to prove, that for eighteen months Mr. Dease, Dr. Arthur, Dr. Gordon, and the writer himself, had been in the habit of treating all ulcers on the penis, whatever their appearance might be, with simple means only, and they all got well. Mr. Guthrie informs us also, that the same plan was pursued at Dover, Chatham, and Edinburgh, as well as by some regiments both abroad and at home. He had also seen the reports of 400 cases treated in the same manner, and with the same success; though it would seem that in many of these cases the cure was very tedious, and the cicatrices of the sores were frequently giving way. Of the secondary symptoms resulting from these sores the cure was likewise tedious, though they were generally of a mild nature; and only two instances of affections of the bones were met with. Mr. Guthrie next proceeds to contrast the result of his practice with mercury, whilst surgeon to the 29th regiment, between the years 1801 and 1809; and he remarks, that during this period, when his patients generally underwent a moderate course of mercury, he very seldom had a case of secondary syphilis; and he is not aware of his having either lost, or been obliged to discharge a man, in consequence of that disease.

In the half year ending the 24th June, 1817, fourteen hundred cases of the venereal disease were treated in the army of occupation in France with mercury, and only fourteen cases of secondary symptoms occurred; whilst of 521 cases so treated in England, ten instances of secondary symptoms appeared—so that the true average proportion of the two numbers united is 1 in 75; whereas in the mode of treatment denominated non-mercurial, the average number of those affected by secondary syphilis was at first stated to be 1 in 10, though, in truth, this proportion was soon discovered to be very much underrated, and there is reason to belive that 1 in 4 or 5 would have been nearer the truth.

Notwithstanding this, however, the non-mercurial plan of cure was extended by degrees to the military stations of England, Europe, and even America, under the sanction and direction of the present Inspector General of the medical department of the army, who has always been among the foremost in promoting every inquiry in which either the interests of humanity, or the advancement of professional knowledge, is concerned. The result has been a collection of reports connected with

this subject, detailing the cases of nearly 2000 venereal patients, whose symptoms, both primary and secondary, had been treated upon the new system. From this mass of information certain conclusions were drawn, and which were afterwards transmitted to the surgeons of regiments, for their information and guidance. From this circular letter it appears, that between the months of December 1816 and 1817, 1940 cases of syphilis had been treated without mercury, of which number 96 had afterwards secondary symptoms of various sorts. Of these 96 patients, 12 were afterwards subjected to mercurial treatment, chiefly for reasons of expedience, rather than of necessity; and even in these cases it was found that alterative doses of mercury were sufficient to effect a cure with several of them. Of the whole number of primary sores, 65 were cured finally by mercury, in consequence either of the slow progress they had previously made, or from their evincing a disposition to spread; though at the same time we are informed, that the non-mercurial practice, both in the primary and secondary forms of the disease, *generally* occupied less time than when mercury was had recourse to. Such was the result of the number treated without mercury.

In the same period of time, 2827 men, with ulcerations of the penis, were treated with mercury; and of these, 51 only had secondary symptoms: but these last appear to have been extremely severe, and more intractable than when mercury had not been used for the primary sore; so that two men were obliged to be discharged the service, in consequence of the injury sustained by their constitutions. Among the general observations with which this document concludes, we must not omit to notice the discrepancies in the reports from several regiments: thus, in one, four cases of secondary symptoms supervened out of twenty-eight treated with mercury, whilst, in another, sixty-eight men were so treated, and not one example of secondary affection was observed during the space of fifteen months, to which space of time this report extends. It is also asserted, that no peculiar forms of secondary symptoms were fairly traced to any peculiar primary sore; that, in cases treated without mercury, iritis had frequently been met with as a secondary affection—sometimes alone, at others in combination with eruptions of various kinds; and in these, mercury was generally resorted to with success: finally, the frequent reappearance of the primary sore, and repeated attacks of eruption, have most commonly been the reproach of the non-mercurial treatment. Another singular circumstance developed by these returns, is the infrequency of syphilis in the West Indies, compared with its ravages in Hindostan: so striking is this difference, that Dr. Good, who has compared these returns, asserts, that every two regiments in the East Indies furnished, at least, as many cases, both of genuine and doubtful syphilis, as are furnished by the whole army in the West Indies; for example, the whole number, in the year 1823, in that part of the world, amounted to 36 only, whilst one regiment in the East Indies afforded 177 cases in the same period.

I should be almost afraid of wearying the reader with these accumulated facts, but I feel it my duty to consider this subject as one entirely novel and unknown; and that, as professing to give an entire and complete body of doctrine relative to the disease, I should not feel myself justified in passing by any series of observations on public record, which tends to put this question in a clearer point of view: but it only remains now to give the result of Mr. Hennen's labours, and this part of my subject will be completed. The substance of what Mr. Hennen has detailed may be thus shortly stated:—The first trials of non-mercurial practice were witnessed by this gentleman at the Hospital at Hilsea, in 1816, under the superintendence of Dr. Knox, where, between the months of May and September, out of 58 cases of primary sore, 28 were healed without mercury. It was not, however, until October 1817, that, being principal medical officer in charge of the district of North Britain, Mr. Hennen had an opportunity of trying this plan upon an extensive scale, and he thus sums up his opinion.

"Every thing I have seen of this practice confirms me in the belief of the possibility of healing primary sores on the genitals, of whatsoever description they may be, without the employment of mercury; and I have met with nothing to make me question the propriety of the trial: of some hundred cases, none have hitherto resisted." But farther on, he adds—"Secondary symptoms occur more frequently, and appear at an earlier and more determinate period than when mercury has been used; but they have not proceeded from bad to worse; they do not exhibit the same violent and unrelenting symptoms which we have observed in many instances where mercury has been used; the eruptions have not run into ulceration; they have not formed into large scabs, or extensive blotches, nor have the bones of the nose, or other parts, been affected with caries." All these points are clearly established by several tables, very perspicuously and accurately drawn up.

From the above mass of evidence the following conclusions appear to be fairly deducible:—1st. That all sores of the genitals, without exception, are curable without mercury. 2dly. That secondary symptoms occur in the proportion of at least one in ten of those cases where no mercury is used; whilst on the contrary, the proportion of such cases is only as 1 to 75 where that remedy has been employed. 3dly. The possibility of curing nearly all forms of the secondary syphilitic symptoms without the assistance of a particle of mercury. 4thly. The mildness of these symptoms, which, excepting in about half a dozen instances, were confined to eruptions in the skin, and ulcers in the throat. 5thly. That the period required for the cure of the primary sores by the non-mercurial plan was not in general greater than where mercury was employed; though it is

admitted that the cicatrices of the sores remain frequently in a state of disease, were often ulcerating again, and that the secondary symptoms, though not violent, were very tedious; and when apparently cured, would not unfrequently recur again and again. I ought here to observe, that the practitioners in France had long been in the habit of curing all ulcerations on the genitals without mercury, though they did not pursue this plan in consequence of direct experiment, but from a conviction that, generally speaking, these sores healed more readily by the employment of simple means only, but they were in the habit of prescribing the corrosive sublimate internally, in very small doses, for the purpose of preventing the attack of secondary symptoms; such for many years had been the practice of Cullerier, of Paris, whilst other of their surgeons relied entirely upon diet drinks, of which sarsaparilla formed the basis.

We may now, perhaps, be tempted to exclaim with an anonymous French writer, "there is no venereal disease at all;" and passing from the extreme of timidity to that of confident rashness, be disposed to place the belief in syphilis in the same rank with that concerning the contagion of the plague, and the existence of hydrophobia, as held by some sceptical philosophers of the present day; nevertheless, such a conclusion would, I conceive, be equally premature in either of these cases, for the experience of a few more years, whilst it has left the facts above cited untouched and uncontradicted, has amply shown that the proportion of secondary symptoms, as well as their obstinacy, the slowness and uncertainty with which primary ulcers heal, their frequently breaking out again under the non-mercurial system, rendered it highly inexpedient, and in fact impossible, to introduce this practice into general use; nay, more, in several instances, even among the military, little accustomed to regard consequences, it began to excite uneasiness; the proportion of cutaneous affections, of ulcerated throats, of pains in the larger joints, and other concomitant evils, became a serious evil, and induced many regimental surgeons to remodel their practice, and to adopt a plan of treatment less exclusive with regard to mercury.

Evils, still greater, but which are not fairly ascribable to the above investigations, also arose throughout the country; for the general confidence in the power of mercury having become shaken, if not destroyed, and nothing like fixed principles established in its stead, many practitioners were satisfied with a very trivial or slovenly exhibition of that remedy; it was often given out without any precaution, and the result was, that a few of those who became affected with primary syphilis escaped some after consequence: this circumstance, formerly so rare, soon produced a reaction in the opinion of professional men, and the new doctrines did not fail to suffer in the estimation of those who had at first been among the number of their warmest advocates, and to this day the practice continues in a state of uncertainty, of which this, I conceive, is no exaggerated picture. Still farther to confirm, and extend this confusion, other circumstances have very much contributed: I allude especially to the inquiries instituted into those diseases resembling syphilis, as well as the recent distinctions drawn by Mr. Carmichael, the direct consequence of which has been, that by endeavouring to distinguish with accuracy the origin of particular ulcerations, and restricting the syphilitic sore to one peculiar form, in relying entirely upon verbal descriptions of ulcers, which no two surgeons perhaps have seen in the same point of view, or in the same *state of their progress*, the practitioner has become involved in a labyrinth of contradictions, and the patient has too frequent cause to lament that his security has been sacrificed to unnecessary refinement.

The direct course of my inquiry now leads me to consider that branch of the subject to which I have just adverted—that is, to diseases resembling syphilis; but before I do so, I would wish to point out the real benefits which are to be practically derived from the investigation into the natural history of the disease of which I have just given you a pretty extended account. In the first place, then, it must be obvious, either that the venereal disease has been sadly misrepresented in former times, or that its symptoms have become much milder, either from the mere lapse of ages, or in consequence of the change which the continued exhibition of mercury from generation to generation has produced. To me it appears very unlikely that our ancestors have made any very gross mistake in their account of the symptoms of syphilis; that occasionally some doubtful affections might be admitted among the number is very probable, but if we take, not the particular opinion of one writer, but the general account of a number of contemporary authors at any period subsequent to the middle of the 17th century, the descriptions they give rather differ from what are now met with in the severity of the symptoms than in their identity; and we must remember, also, to deduct from this account all those consequences which are universally admitted to have been produced by the profuse and very incautious manner of administering mercury, at that time in use; it is, therefore, I think, nearly as improbable that mercury can be allowed to have the merit of having modified or lightened the symptoms. Had its character, as a specific, been indeed so absolute and undeniable as been more than once asserted, we might have expected the disease to have become extinguished rather than modified; whereas, as far as we can collect from authors, or from our own experience, whenever mercury has been given without effecting a cure, so far from the disease having any tendency to become milder, it has been actually aggravated. We are, therefore, reduced to embrace the only remaining supposition, that the progress of time, bringing with it a better and more wholesome mode of living, both with respect to food, clothing, and lodging,

together with much greater cleanliness of person, and a more discriminating and temperate plan of treatment, have been the real and efficient causes of the milder aspect of the disease in these latter days; though, perhaps, after all, we are boasting of what may only be a temporary blessing, for I would suggest the probability, that at those particular periods in which we have found practitioners abstaining from the use of mercury, as, for example, in the days of Fallopius, Abercrombie, and afterwards of Morgagni, and many others before and since, there is reason to suppose that they did so in consequence of having had to treat a milder form of the disease, just as in our own day we have seen one surgeon speaking of the disease in Portugal as very severe, whilst a few years later that severity was not recognised; and still later it has been observed, that in Ireland primary sores of great malignancy have been met with in some seasons which have been unknown at others; therefore, whilst I admit the fact that syphilis is much milder now than formerly, that is, I mean within the memory of practitioners now living, yet I think it by no means impossible that this condition of things may not endure, and that more severe forms of the disease may again become prevalent, in the same manner that the small-pox epidemic shall remain mild and mitigated for some years, and afterwards return to us with renewed violence. However this may be, it may fairly be asked, granting that the disease is now mitigated in severity, what has the profession gained by the experiments above mentioned? and to what practical purpose can they be applied? My answer would be, in this point of view they are invaluable, since they have shown us that we may safely, nay, advantageously, dispense with the use of mercury upon all those occasions wherein we discover, or suspect that it is operating deleteriously upon the constitution. Whenever fever is excited, or pains, either local or general, are induced, without apprehending any of those formidable consequences that used formerly to alarm the surgeon as well as the patient, we may await patiently and tranquilly the favourable moment for exhibiting the medicine; we may apply to the ulcers on the genitals the same principles of cure which would be applicable to sores on any other part of the body; nay, more, in those constitutions prone to struma, we may confidently forbear its employment, or when necessary to do so, we may prescribe it either in so mitigated a form, or under such combinations, as to disarm it from all those dangers which occasionally render its exhibition a cause of more real suffering than the disease itself; and yet let me not have it imagined that I am one of those who recommend the exclusion of mercury from practice in the venereal disease; on the contrary, it is my object to prove that in the vast majority of cases it is our sheet anchor.

Those who recollect the summary manner in which all breaches of surface on the parts of generation, were, at no great distance of time, condemned to mercurial treatment, without any reference either to the condition of the sore or constitution; the frequency with which sores so treated were accustomed to inflame and spread, instead of healing; the fever that was occasionally lighted up; in short, the combat excited between the powers of nature and a mistaken line of practice, may be inclined to wonder that no author, prior to Mr. Hunter, should have attempted to draw any distinction between the different species of ulceration met with on the parts of generation; the more especially, since the fact of some of them being aggravated by the use of mercury, was at that time universally admitted, and acknowledged to be a conclusive proof of the nature of the affection.

From the time of Mr. Hunter's publication, then, a new page of our history may be said to be opened; until then syphilis was not doubted to be one disease, and all the variety of symptoms were attributed to one poison; but from that date a new host of diseases became acknowledged and admitted into the catalogue of human woes; these were said to resemble lues in appearance and progress, but yet they were thought not to be syphilitic. This, then, is the next subject that demands our attention, for this is in truth the foundation upon which Mr. Carmichael has built his theory of a variety of syphilitic poisons.

Now, although I am inclined to admit that good has in many respects followed the investigation thus commenced by Mr. Hunter, and that many complaints, which were formerly confounded with syphilis, have since been discriminated from it, and some progress made towards a more accurate classification of the symptoms, yet it cannot fail to be observed that much of the reasoning employed by Mr. Hunter, and subsequently by Mr. Abernethy, relative to diseases resembling syphilis, falls to the ground, since the fact of all forms of primary ulceration being curable without mercury has been admitted; for all their distinctions are built upon the converse of that proposition, and with regard to the term pseudo-syphilis, first employed by Mr. Abernethy, I must beg to observe, though perhaps the remark is rather out of place here, that I consider it as a term most unfortunately chosen, since it cannot fail to lead to a confusion of ideas, and as long as it is employed must rather tend to prevent than facilitate a discrimination so much to be desired, for these diseases are either syphilitic or they are not; and, therefore, at once to assert they are not so, and yet to employ a term that brings the actual name of the original disease to the mind, cannot fail to create and perpetuate confusion. But to return from this digression. Now, although the belief of the existence of diseases simulating lues venerea is repeated by almost every modern writer upon this subject, so that Dr. Good has even given them a distinct place in his nosological arrangement, I do not hesitate to declare that I do not believe in their existence, and I cannot conceive that we are justified in drawing any such marks of distinction now that we have seen that syphilis itself, ac-

knowledged and undoubted syphilis, under all its forms, is curable without mercury. When that fact was either unknown, or denied, it certainly became necessary to seek some escape from the dilemma which occasionally presented itself on finding certain symptoms, so similar to those of syphilis as not to be distinguishable from it by the senses, getting well either with sarsaparilla or without it; or again, other symptoms aggravated instead of being cured by the action of mercury. But surely we have now learned, by the thousands of experiments that have been made in this country and on the continent, that this distinction is not founded on facts, that all forms of syphilis may get well without one particle of mercury, and that under peculiar circumstances, that mineral may act as a poison, although the disease for which it was prescribed was undoubtedly syphilitic.

There appear to me to be three questions connected with this branch of my subject, which it would be very desirable to decide:—1st. Whether it is possible to ascertain by the appearance and progress of the ulcers on the genitals, if they be the produce of impure connexion or not? 2dly. Whether breaches of surface on the parts of generation not produced by sexual connexion, are ever known to be followed by constitutional symptoms of any determinate character? and 3dly. Whether sores acknowledged to be the result of impure connexion are regularly and invariably succeeded by peculiar trains of constitutional symptoms, having constant reference to a peculiar form of ulceration?

Towards deciding either of these three questions I am afraid it must be admitted that Mr. Hunter has not done much; he has certainly the merit of having first opened the road to future inquiries, but the cases he has brought forward in support of his opinion admit of a very easy solution now, and demonstrate the very rapid strides which have been made of late years in the knowledge of this class of diseases. In order, therefore, to trace the progress of this inquiry, it will be necessary for me to mention, shortly, the principal facts which Mr. Hunter has adduced in support of his views relative to diseases resembling syphilis, and we must recollect that his observations do not apply to herpes of the prepuce, to common phlegmon, or to erysipelas, which may attack the parts of generation as well as any other portion of the body, and of which affections he treats separately. Mr. Hunter commences by remarking that many diseases resemble each other in one or two of their symptoms; and that, therefore, in order to draw a just judgment, the aggregate of the symptoms should be considered, and this observation he deems more applicable to the venereal disease than any other, since he conceives that it has no one symptom peculiar to itself; and this he attempts to illustrate by the example of a gonorrhœa; but the most remarkble passage relating to this question is the following:—1st. That sores on the glans penis, prepuce, &c. in form of chancres, may and do arise without any venereal infection; and again, other disorders shall not only resemble the venereal in appearance but in the mode of contamination, proving themselves to be poisons by affecting the part by contact, and from thence producing immediate consequences similar to buboes, also remote consequences similar to lues venerea: the inference, however, which he draws from these two positions leads us to the belief that the only criterion he admitted between a venereal and a non-venereal disease, was the possibility of curing one of them by mercury, and that whenever it happened that the symptoms went from bad to worse under its use, he supposed that he had been mistaken in the nature of the case. That this is a plain statement of the fact the relation of a few of his cases clearly demonstrates; the first is that of a gentleman in the West Indies, who having a wound in his finger, opened the abscess of a negro woman who was labouring under the yaws, and was conscious at the time of having inoculated himself; he had recourse to mercury, but in spite of it successive tumours formed over the hand and up the arm; in a month or two nocturnal pains came on, with other distressing symptoms, which persisted, although he used mercurial frictions for five months; afterwards, at the distance of half a year, a scabby eruption appeared over his legs, and his tumours ulcerated: the nocturnal pains being then mitigated, he never could bring on salivation, though the mouth was tender, and he arrived in England about two months later, where he obtained a cure by the use of mercury and sarsaparilla conjoined. You will perceive at once that this is not a case of the venereal disease, and has nothing to do with the question; the disease was the yaws, and ran its course in the manner usual with that complaint.

The second case is that of a gentleman, who, after undergoing a course of mercury for the cure of chancres, was restored to health in five weeks; he almost immediately had connexion with a woman; in a few days the prepuce appeared as if chapped all round the edge of its reflection. The connexion was, notwithstanding, continued, and the patient applying at length to Mr. Hunter, the chaps or fissures were found to be very deep, and paraphymosis had taken place. In this dilemma, Mr. Hunter considering the case not to be venereal, sent the patient into the country, and his sores all got well without any thing being done for them; but a fortnight afterwards the lady became ill, and after a slight fever had a swelling in the groin; its progress was slow, but it broke, and as it showed a disposition to heal, Mr. Hunter did not consider it as venereal; but at the end of six weeks, when it was perfectly well, eruptions came out on the skin of the face, thighs, hands, and feet. This staggered Mr. Hunter a little, but they got well, although nothing was done. Surely this is a case about which we should not be much puzzled now: a man excoriates himself violently, he continues to have connex-

ion, he becomes infected, the female in a very short time proves herself to have been infected by the appearance of a bubo: it is not even hinted that an examination took place to discover whether ulcerations in the pudendæ existed or not, and in truth the whole curiosity of the affair is, that all the symptoms got well without mercury.

The third case is simply one in which the patient's health (he was a man of intemperate habits) was much affected, so that on prescribing mercury for a sore on the glans penis, attended with excessive pain, it was found to disagree, and the sore was finally healed by cinchona, sarsaparilla, and opium. This was followed some months after by a tumour of the scalp, and succeeded by an extensive caries of the cranium, attended with excessive pain; these sores healed up, and others ensued, which all got well, excepting that for a long period one large ulcer at the angle of the right eye remained unhealed, so that in this case also there was nothing but what the recent experiments above recited render perfectly intelligible; for here was evidently an irritable habit of body, which, combined with an improper use of mercury in the first instance, produced a hybrid disease, which has in most respects more the character of struma than of syphilis, and which indeed receives a very rational explanation in the following passage of this author's own work: "The venereal disease often becomes the immediate cause of other diseases, by calling forth latent tendencies into action." It is, therefore, I think, very evident that Mr. Hunter leads us but a very little way towards the solution of either of the questions above proposed, but a much more ample field opens upon us when we come to examine the works of Abernethy, Evans, and Carmichael. This task I reserve to my next essay.

(*To be continued.*)

From the Edinburgh Medical and Surgical Journal.

ON THE SANGUINEOUS TUMOUR OF THE FEMALE BREAST. By John Rodman, M. D., Paisley.

The disease which forms the subject of the present paper is one in which the medical profession must be much interested. Peculiar apparently to the female breast, it asumes a very anomalous character. From its locality it may be referred, according to Cullen's Nosology, to the class Locales; and from certain circumstances to be mentioned in its history, it may be thought to rank in the order *Tumores*. But it presents several peculiarities in its morbid structure and general history which entirely exclude it from a legitimate place with any genus or species otherwise distinguished in this arrangement.

Neither am I aware of any other mode of classification yet proposed in which it can be satisfactorily included. Even the attempts made to refer it to the class of encysted tumours, (*tumeurs enkystées, loupes, cystides,*) the contents of which are surrounded by a bag or cyst, have been found insufficient for embracing it, encysted as it is, from difficulties which will fully yet appear. To Mr. Abernethy's method the same difficulties apply; so that the principle of arranging tumours from their anatomical structure is in this instance inadmissible.

The usual seat of this disease in the female breast, and its resemblance to several of the malignant affections incident to that gland, must make it always be viewed with a suspicious eye. This difficulty in distinguishing its nature is augmented by the uncertainty of the principles on which its treatment has been hitherto conducted, and renders it of the utmost importance to the practitioner to determine what method of management is most likely to be, if not favourable, at all events least injurious. The opportunities which I have enjoyed of observing its disposition and character in different stages of progress, and some experience in its treatment, have led me to pay attention to its peculiarities; and it is with the hope of contributing to the alleviation of a malady accompanied not only with bodily pain, but much mental anxiety, that the following observations are now communicated to the profession.

The first distinct examples of this malady were published by Dr. Monro Primus in the fifth volume of the Edinburgh Medical Essays and Observations, under the title of "Histories of Collections of Bloody Lymph in Cancerous Breasts," with the reason for giving them publicity, namely, "no mention being generally made by chirurgical writers of a collection of bloody lymph in the breasts of women, as a consequence or attendant of the schirrous tumours of those glandular parts."

His second and third cases were of middle age, the first was about fifty, and the last about thirty-seven. In the first case, the tumour on the exterior of the breast was large; in the second, it was increased to a great bulk; the third, increased considerably; and the breast of the fourth was of itself very large. Of the first, the tumour was hard and unequal. The patient of the second had been two years sensible of a hard tumour. The whole breast of the third seemed to be schirrous; and that of the fourth was hard. The discharge from the first and second was bloody water, from the third blackish-red lymph, and the fourth bloody lymph. The first broke after the use of cataplasms; the second was opened, and another incision made next day; the third and fourth were opened with a very small lancet. The breast in the fourth case was amputated; and that of the second was after death dissected, and found to be an empty bag without any tumefied gland in it. Three died, and one was abandoned as hopeless.

The physical characters of the tumour varied a good deal. The second case had turgid cutaneous veins and cuticular redness; the third very large and varicose veins, the integuments at last turning red; the fourth had varicose veins of the skin, and the nipple shrunk

out of sight; and, with respect to painful sensations, the first and fourth cases had sharp, the third pricking, the second lancinating pain, in the last more violent after being opened, with supervening gangrene.

The inferences which these cases suggest I shall consider afterwards. Meanwhile, the analogy of Richter's case, (Observ. Chirurg. Fasc. 3; also Trans. by Spence,) and of one by Mr. James Briggs, Edinburgh, with the cases recorded by Dr. Monro, is such as to render it proper to insert them ere entering upon the proposed considerations.

Richter commences by stating, that "a lively healthy woman, 60 years of age, with a good appetite and digestion, used formerly to observe at the time of her menses a hemorrhage from the mammæ. She had never had children. After the cessation of the menstrual flux, the hemorrhage from the breasts still continued for a time, only it did not appear so regularly at certain periods as before.

"But at last another phenomenon made its appearance without any particular cause. The whole breast began to swell, and finally increased to a great size. It did not feel hard, but elastic, and equally so in every part. Fluctuation was nowhere to be felt. With regard to schirrus, of which I had been so much afraid, I was now perfectly at ease. Neither was the breast upon the whole painful.

"At last a place at the side of the nipple became very prominent, tense, and red, which, however, was not in the least painful, and after awhile threatened to burst of itself. I opened it with a lancet. To my great astonishment not a drop of purulent matter appeared, but only a great quantity of blood, partly coagulated, partly fluid, partly black, and partly pure red.

"As, on account of the great quantity of blood, some of which was apparently fresh, the great age of the patient, and surprise occasioned by the unexpected sight of the blood, I had reason to fear a syncope, and great debility, I stopt the discharge, and covered the wound.

"For four days I removed the bandage twice a-day, and each time a quantity of blood of the above description issued out. After this the discharge changed to a reddish watery ichor. The preternatural swelling of the breast was now gone. The schirrus had remained unchanged during the whole process.

"The ichorous discharge continued for six weeks; but at last it became more purulent, and the schirrus gradually lessened, till at the end of that time it was entirely gone.

"The abscess then gradually contracted, and at last turned into a small, narrow, shallow fistula, which remained open many years, indeed as long as the patient lived. It generally discharged only a small quantity of ichorous fluid, but sometimes a considerable quantity of pure blood.

"As the woman with this discharge felt herself otherwise well, and as I was afraid that an evacuation to which she had been accustomed for so many years could not be stopt without danger, I had not the courage to attempt any thing decisive in order to obtain a complete cure."

Mr. Briggs' case of sanguineous tumour of the breast, which is dated 1810, occurred in a single woman, aged 45, and extended from near the nipple towards the axilla; was stated by her to have eighteen months before origi nated like a small moveable kernel under the skin; and was sometimes painful on the arm pressing upon it during sleep. Fluctuation perceived for three weeks.

Being punctured, four ounces of dark and inodorous bloody fluid abounding with serum, and not coagulating spontaneously, were discharged. Repeated discharges of blood by the orifice bursting open at intervals for thirty-one days, and becoming more frequent and profuse, induced her to submit to the removal by excision of the diseased part; and in about six weeks from the time of the operation the parts were perfectly healed. This woman continued in apparently good health for eight years afterwards without any appearance of relapse of the local complaint, and died of sudden illness in 1818.

"In the centre of the tumour, when examined, a cavity was found capable of holding three or four ounces of fluid. The inner surface presented a number of rugæ or irregular folds, not unlike those of the corrugated inner surface of the bladder, or the auricles of the heart, forming small interstitial cells or foveolæ communicating with the principal cavity. The entire parietes of the cavity, which were about one-third of an inch in thickness, as well as the small cells formed in the sides of it, were found to consist of depositions of successive layers of coagulable lymph. A small portion of the glandular part of the breast had undergone some change in its texture, but had no communication with the cavity before mentioned; and the change probably arose from pressure, in consequence of its contiguity with the principal diseased part."

The similarity of the principal phenomena in these two cases and the four preceding them, will entitle us to consider them as one and the same disease. That there is an appearance of diversity in some of the collateral circumstances, however, will not be denied. This appearance, therefore, lest it lead indirectly to any difference of opinion, renders a fuller investigation of the disease, and more extended acquaintance with its general characters indispensable. This purpose I shall endeavour to accomplish, first, by laying before the reader several cases of the disease; and, secondly, by attempting a general description of its progress and phenomena.

Case 1st. An unmarried lady, near the age of 60, at a distance from me of more than eighty miles, while she was amusing herself in the garden, placed her foot upon the iron head of a garden hoe, by which the small transverse wooden handle struck her on the left breast. This happened in the summer of 1813. The pain at the time was sharp, but moderate, subsiding in the course of a quarter of an hour.

She soon banished it from her mind, and thought no more of the subject till the following December, when she discovered unexpectedly some hardness in the body of the breast. Next morning a physician of extensive experience, who was consulted, ordered a leech and Goulard embrocations, considering it of little consequence.

I saw her for the first time in 1817. The tumour was fully an inch and a half in diameter, and had attained what I shall afterwards distinguish as the third stage. She had enjoyed a vigorous constitution, and possessed a lively, acute, and active mind, with very superior information; but her general health was greatly impaired. The history given me of the second stage did not precisely correspond with that which is related in this paper. A considerable variety of external means, by different advices, had been employed with a view to suppuration.

For a long time in the first stage there was no pain, and little or no sensible increase of swelling in the part. It was only within the last six or eight months that she had at times been distressed with the suspicion that the disease was cancer.

My first object was the restoration of her general health, which seemed of the greatest importance. For that purpose, better regimen and a more methodical and steady attention to due exercise in the country were prescribed. Besides attending to these prescriptions, she was directed from time to time to persevere in the use of tonics from the mineral and vegetable kingdoms, especially the former, or those of a chalybeate nature variously prepared; likewise carefully to watch over the alvine dejections, and to use appropriate aperients more regularly, in order to rouse the excretory functions from a state of habitual and neglected inactivity.

For the tumefaction of the breast various instructions were at different periods requisite. The liniments were diversified according to circumstances, in different degrees of strength, in different proportions of camphor, or camphor and opium, and similar remedies, with which, in the directions transmitted, they were frequently compounded. Moderate pressure, by means of thin sheet-lead properly applied and retained by bandages, was also from the first recommended, in consequence of many years experience that this was occasionally useful in certain varieties of mammary tumours.

This lady recovered her health and strength considerably, and for nearly two years the disease neither gained ground, nor seemed to differ materially from its state at my first inspection. I saw her last in April 1819.

A little prior to this, while enjoying herself with some friends, she received from extraordinary and very active exertions, an injury, which caused the tumour so speedily to increase in bulk, that before I could see her it had burst, and was discharging its bloody contents. Her medical friend expecting, as in some abscesses, to accelerate reunion, enlarged the sore by incision. In this, however, he was disappointed; for after employing divers judicious means, he failed to excite the healing process; and the patient gradually sinking from the continued discharge of the open sinuous cavity, died about eighteen months after, without any painful or distressing sufferings, as I was informed, beyond the exhausting effects which accompany protracted ulceration.

Case 2d. While a widow lady, distinguished for literary talent, was alighting from a carriage soon after the summer of 1816, the hand of the servant, while assisting her, pressed incidentally upon the right breast. The pressure occasioned a degree of uneasiness for some hours. She was at this time about fifty.

My attendance commenced in July of the subsequent year, when the disease was advancing to the latter periods of the first stage. She was subject to dyspepsia. The treatment was similar to what is mentioned in the preceding case; and till the middle of 1822, the disease, in place of making progress, appeared by the means that were employed to be rather receding, and gave no pain; nay, almost no trouble.

At this time, in consequence of falling from a horse, the clavicle of the same side was fractured. From this injury she recovered in the usual time, no direct impression upon the breast having been perceived. Yet it felt more tender, more sensitive, and more easily affected after this accident.

By the end of March 1824, the tumour was so much enlarged, that its central diameter was equal to two inches and a half. She now came to reside in my neighbourhood, and for the first time I emptied it of its dark, grumous, bloody, and variegated contents, by introducing in a lateral direction an instrument resembling the couching needle. The aperture uniting and soon closing up, I kept it open by introducing night and morning the sharpened point of a silver probe for twelve days successively, during which the cyst contracted, and the integuments had become in feeling and appearance almost natural. In this way I was able to remark that the discharge varied on different days, and at different periods of the same day; being sometimes thick, clotty, and dark coloured blood, sometimes oily, yellow, and watery, like serum, according to the corporeal exertions of the patient.

She returned home, and without enjoying, as formerly, the opportunities of attending to the necessary treatment. I had occasion from the re-accumulation of the fluid contents afterwards to pierce and evacuate the tumour four times, twice in May of the same year, once in June, and once in August. At three of these times it appeared to me proper to treat the complaint on the principles of treating hydrocele, by emptying the sac, and then injecting port wine by the small or narrow aperture. The first time the wine was scarcely equal to the quantity of water that was mixed with it, for I feared the possibility of high inflammation; but neither pain of any consequence nor

inflammation ensued. The second time the quantity of wine was equal to that of the water, and the third time it was a third more. The pain excited by this injection was such for a little as to occasion a short nervous paroxysm; and the local inflammation was more obvious than ever before, but by no means excessive.

Five weeks after this, the cyst felt contracted like a small moveable body somewhat of a fleshy firmness, so that hopes were entertained that a cure was effected. Yet the fluid again collecting about the close of that year, I committed the case to the care of another practitioner, by whom, in February 1825, the breast was extirpated.

It was filled at the time, as he informed me, with the usual contents; and on inspecting the amputated breast, the fluid was discovered in two large cavities, having capsules or cysts exceedingly thin and delicate. It appeared during the operation that there was but little remaining of the glandular part; and with this was mixed a small portion of pus, not unlike that of a strumous nature. The structure of its body was formed chiefly of adipose and cellular substance—there were no varicose or distended blood-vessels, or schirrous tumours, but six separate spheroidal bodies of the size of garden peas, filled with limpid fluid, with some appearances of a spongy nature nowise redder than the adjacent parts.

The lady has now been nearly two years in good health.

Case 3d. The third case is that of a lady, the mother of a numerous and healthy family, herself peculiarly active, and favoured with an uncommon share of excellent health.

Her attention was first directed to the examination of the right breast, by observing appearances, time after time, of blood upon the linens. At length she discovered it issuing from the nipple; and in the course of some months perceived extending from its base a firmness of substance.

I saw her not long after this in May 1819, when her age exceeded seventy-six. No distinct or insulated tumour was then perceptible. The body of the breast was well formed and of good bulk; atlantal and dextral to the base of the nipple there was circumscribed tumefaction of some degree of firmness, accompanied with scarcely any uneasy sensation. The nipple indeed had been frequently very tender, tumid, and painful; and when most painful, it was seemingly sufficient in its inflamed and irritable state to account for this adjacent swelling, which was invariably then most considerable. It seems not unlikely, from several circumstances, that the nipple and anterior of the breast at some former period sustained external injury, of which she retained no accurate recollection.

The disease was only in its commencement, or merging into the first stage.

The morbid state of the nipple and adjacent parts became for a long time the main object of medical treatment. This embraced preventive as well as curative means, to the proper comprehension of which it is only necessary to state, that whenever she was affected by exposure to cold, which not unfrequently revived pains of a rheumatic kind of the same side, the nipple became disposed to the discharge of blood, appeared inflamed and tumid, feeling sore and tender, especially if, as often happened, the bowels were at the same time more than ordinarily constipated. The regulation of diet, of exercise, of temperature, and the exhibition of appropriate laxatives—neither drastic, however, nor mercurial—besides the topical applications enumerated in former pages, were therefore most obviously requisite, and, in conjunction with strict care in the mode of dressing, were attended with very happy effects.

Such was the tardy nature of the disease under this treatment, that it was not till a short while previously to October 1822, that it attained the second stage; and the third stage was only so far advanced in April 1824, as, after some objections and hesitations on the part of the patient, rendered it necessary to urge her to have the tumour pierced, its central diameter being about two inches.

This I effected in the same manner, and by the same instrument, as related in the case preceding. The discharge consisted of dissolved blood free from any fœtor, of a glutinous, clammy nature and dark brown colour, with oleaginous globules on the surface—no pure blood.

After discharging the contents frequently, as in the history of the above mentioned case, for three weeks, the wound, which generally closed in a few hours, being for some days united, the parts having assumed a very natural appearance, and she being without pain or other complaints, except from the tenderness of the nipple, nothing more than precautionary treatment was farther required for thirteen months.

About the conclusion of that period the sac was again filling; for some time previously another tumour, small, yet similar, had made its appearance on the opposite side of the breast. I found it therefore necessary to pierce the sac a second time in May 1825, when, as in the case before referred to, I also had recourse to an injection of equal parts of wine and water; but without exciting either the pain or the inflammation which was wanted.

It was on the 13th of May these steps were taken; and yet there was no abatement of bloody effusion within the sac from this period till the 12th of the ensuing December. On the 15th of May, I was obliged again to commence the process of drilling with the probe, as lately noticed, in order to open the tender aperture, and evacuate the dark coloured venous blood with which the sac was distended. Indeed, so frequently was this process afterwards required, that I was under the necessity of performing it eighty-three times by the day of December above stated; so that, by calculating the bloody and serous discharge at the average of three ounces each time, although its quantity was often greater,

the amount of the whole for these seven months is believed not to be much short of twenty-one pounds, or nearly three gallons.

The weather of the latter date was particularly damp, cold, and chilly. Her apartment had been suffered to become unusually cold during the night; and she was suddenly attacked with rigours, and subsequently with fever of a rheumatic tendency, which increased for three days, and was only subdued at the end of a week.

In less than two days the whole breast became swelled and inflamed; and as the sac continued to fill, I was obliged to have recourse again to repeated perforation with the probe. For three days at first the discharge was a mixture of blood and pus, but almost ever afterwards it was wholly purulent. The evacuation of this matter always relieved the local disease, and therefore I continued the use of the probe in opening the same aperture for that purpose, night and morning, nearly six weeks, the quantity of matter discharged towards the end becoming less and less.

In the beginning of the following February, seldom did any kind of discharge take place from the use of the probe; for its use was persisted in till the breast was thoroughly healed. During the continuance of the purulent, as well as the sanguineous discharges, every possible means were used to support the patient's strength. This was accomplished by prescribing variations of palatable and nutritious diet, as well as frequently repeated, but moderate, doses of vinous or spirituous liquors diluted, along with various preparations of cinchona.

Her health was now so completely restored, that the breast having assumed its natural and full appearance, without ulceration, she was able for four months to go about pretty freely, to ride in a carriage, and even in a considerable degree to employ herself in several active occupations.

Early in the following summer, she was once more attacked with fever of the rheumatic type, from which she recovered; then towards the end of August with a species of cholera.

Although she recovered from the main symptoms of this last disease, her strength was so exhausted by it, that she was afterwards in great measure confined to bed. Great debility was her chief complaint; and on the 24th of September of the same year, 1826, she suddenly and unexpectedly expired, having passed her eighty-fourth year, and lived nearly nine years from the commencement of the breast complaint.

Case 4th. It may not be improper merely to advert to another case of encysted tumour in a lady of about thirty, and a constitution more than commonly vigorous. When I first saw her she had observed the tumour of the right breast, then nearly an inch in diameter, above fourteen months. All the symptoms indicative of its merging upon the third stage were very manifest. Her youth, health, and vigour appeared favourable, however, to the hope of promoting suppuration. This was accomplished by employing the usually internal means of invigoration, and external suppurative remedies for seven weeks, during the greater part of which she was persuaded to remain for that purpose mostly in bed. At the conclusion of that period the part was opened, and I had the happiness of seeing it healed up in little more than the usual time.

From all the circumstances in the survey of the four cases of Dr. Monro, it is natural to ask, is each of the discordant symptoms to be considered as necessary to constitute the specific character of the disease? Part of the morbid results no doubt succeed the mode in which some of the cases were treated.

Although the tumefaction be considerable, its precise magnitude is uncertain; and this peculiarity is another perhaps of the instances of the imperfection of every classification of tumours which has made its appearance. It is only when a cyst has been formed in the mamma, which contains a fluid partaking less or more of sanguineous constituents, along with other circumstances applicable to the disease, that the tumour can be regarded as that collection of bloody lymph in the female breast, which distinguishes the disease under consideration.

It is observed by Mr. Benjamin Bell, that "It is not what a tumour may eventually become, but what it is on its first appearance, that can admit of any description." Yet the tumour of which we are speaking is one which must be an exception to such a rule. Its first appearance, when compared with that of encysted tumours generally, would on no account justify the conclusion, that, at a future period, it will become an encysted tumour containing nothing but fluids of a bloody consistence.

It is only necessary, in proof of the difficulty of concluding from first appearances, or symptoms most obvious at the commencement, to refer again to the cases Dr. Monro has recorded. In the first case there was a large tumour; in the second the tumour had gradually increased to a great bulk; and in the fourth case, when the Doctor first saw the patient, the breast was very large. But the tumour in the first case was not only large, it was likewise very hard and unequal; the second was also hard; the third, from a small hard tubercle, proceeded, ere fluctuation was perceived, till the whole breast seemed schirrous; and from a small red tumour in the fourth case, the breast in which he felt fluctuation was large and hard.

I must mention, however, that in no case which has come under my observation, has there been a hardness such as that which seems to have been discovered in these cases. In no instance was there ever any kind of tumour which, strictly speaking, was entitled to the denomination *schirrus;* or which had any thing resembling the firm whitish bands, such as are described by Dr. Baillie in his Morbid Anatomy. Neither have I at any time

or stage of the complaint been able to perceive in the tumefied part any of the circumstances which Mr. Abernethy considers necessary to characterize carcinoma, and to distinguish it from other sarcomatous tumours. Nor does it seem compatible with the laws of the animal processes, that a portion of mammary substance, hardened into genuine schirrus, and completely disorganized, should of itself melt down, and become a sac filled with inodorous, mild bloody fluids.

The contents are often variable both in constituent parts and in consistence. At one time the tumour contains a dark-coloured, grumous, thick, and tenacious bloody matter, which has no doubt given rise to the denomination sanguineous. At another time, the proportion of serum is so considerable as greatly to alter the character of the whole in consistence and fluidity, and to entitle it to the appellation sero-sanguineous. Again, there is frequently little, nay, often none of the colouring matter of blood, the serum being almost the alone constituent, thin and attenuated, from the great deficiency in the relative proportion of albumen. These variations, as has been noticed, are not to be found in different cases only, but also often in the same cases at different times; and even at different times of the same day in some instances,—the discharge in the morning being bloody, thick, and grumous,—and in the evening attenuated serum, having scarcely the tinge of blood; and *vice versa*. From the contents of the tumour, therefore, it is impossible to draw any essential character.

The contents of the tumour are liable to considerable change in the progress of the disease. This change I have observed to occur, first, from the nature and kind of the external applications used; secondly, from the degree and kind of exertions in which the patient engaged; thirdly, from the temperature of the season and circumstances of exposure; and lastly, under various degrees of healthy and distempered conditions of the general system. These changes form instructive indications, by which the means of cure were indicated.

It is of some importance to advert to the local injury which the parts sustain, from the effect of long-continued and increasing distention. Without entering minutely into the consideration of the manner in which such violence is known to disturb the sound organization of the integuments, so as to induce increased morbid disposition, and destroy the tendency to the process of reunion, we may acquiesce with Mr. James in the remark, "that too little is known of inflammation of cysts of chronic abscesses, or diseased action which causes the formation of such cysts, and the secretion of the fluids which distend them, to speak with confidence upon the subject." But, while all this is granted, who can deny the pernicious results of excessive distention in the cases of encysted tumour to which we are now alluding? Independently of every other consideration, the eye of experience, and what is usually termed common sense, cannot be blind to the baneful consequences.

It may indeed be asked, what, then, is to be done in such a case? If I am correct, I believe it is an observation of Mr. Hunter's, that, while in genuine abscesses the suppurative inflammation diminishes from the moment they are opened, on the contrary, the opening of encysted tumours is frequently the commencement of high inflammation. Resting, therefore, upon the authority of one, to whose observations the highest value is attached, practitioners may be deterred from opening tumours such as those under consideration. Whether this has been the case; whether practitioners have often been swayed in former times by a like opinion, or continue still under its influence in treating the tumour we are now considering, may be left as matter of future inquiry. The main point at present is, have any proofs appeared from the few cases on record of any good being effected by allowing the contents of the tumour to become very abundant,—to accumulate till it burst spontaneously? And have we not the strongest reason for suspecting that Mr. Hunter's view may have arisen, either from some inadvertence in the mode of opening them, or from some mistake in the time of opening them, when we know that encysted tumours are daily opened now with impunity?

The observations I have had an opportunity of making during the progress of the cases detailed above, induce me to divide the disease into three stages,—first, That stage which is distinguishable by a certain kind of almost general swelling and disturbance of the mammary organ, previously to the actual formation of any distinct or insulated tumour; secondly, the stage during which a distinct and permanent tumour can be readily ascertained; and, thirdly, that in which the tumour may without hesitation be pronounced to contain an accumulating fluid.

By thus dividing the progress of the disease into three stages, the description will accord with the natural order of morbid phenomena as they occur, the history will be rendered more intelligible, and the matter treated of will be more thoroughly comprehended in all its bearings. In tracing the history of the disease on these principles, I avoid all reference to the term cancer, as I conceive no proof of its alliance with that malady has been yet adduced.

First, then, the disease commences with a gradual and moderate swelling of the mamma, not altogether unlike that periodical fulness and distention which, in delicate females, so regularly recurs in it, from what is called the sympathy or periodical influence between the breast and the uterine system. If all things be duly considered, however, there will be little difficulty in distinguishing the one swelling from the other, when it is known that in every instance the supervention of the former is always more speedy or sudden than that of the latter swelling, sensibly more considerable,

attended with sensations of a nature more uneasy and stationary, more acute, and consequently upon the whole more troublesome.

It ought at the same time to be carefully distinguished from every kind of inflammatory and acute active swelling, to which the breast is so much disposed from morbid impressions in general. This also may be found a matter of little difficulty; for continued sharp pain, distressing tension, inflammatory redness, great heat, or throbbing, or feverish accessions, are symptoms of morbid activity with which it has never, so far as I have witnessed it, been accompanied.

But that it is a swelling which arises from a certain disturbance of the local functions will probably be granted from this consideration, that its immediate cause is often external injury of the parts. In two of the cases with which I have been concerned, its origin was traced either to a bruise from a certain degree of temporary pressure that was nowise violent, or to sudden collision with a solid, though not very hard or heavy body. None of the cases, it is necessary always to keep in mind, exhibited a disposition to any thing like a painful or severe affection. They were of such a mild nature, that the effects were more correctly entitled to the character of occasional uneasiness, than to that of very troublesome or distressing feelings; a circumstance which not only regards the time of infliction, but even the state subsequently, and appears to be one that may be viewed as almost peculiar to this disease.

Hence, if it be admitted that Carmichael, Smith, Pinel, and Bichat, in the arrangement proposed by them, on the principle of referring to the elementary tissues certain morbid processes included under the general idea of inflammation, intended likewise to comprehend the modified process of functional disturbance which has just been noticed, as the result of a particular kind of external injury, there will possibly be less objection to my considering this process as a species of chronic inflammation, peculiar in such circumstances to the cellular tissue of the female breast. Perhaps, on this admission, the hardness of tumour recorded in Dr. Monro's cases may be accounted for, by allowing that the primary or subsequent affection may have been accompanied, at particular times, by a kind of inflammatory operation, whose action, allowing that it was modified, and of a mitigated nature in general, was at these times deeper and more extended than in the cases which have come under my observation.

My reason for this supposition is, that during the first stage of the disease of which we are speaking, there will, for a short while, occasionally be observed, as if it were deep in the substance of the breast, a gradual approach towards a superficial firmness, resembling in some degree that which occurs from recent and temporary renewals of mild or chronic inflammation, and extending more or less considerably; and it is a symptom indicative of this stage being nearly terminated whenever the firmness begins very easily and with more than usual frequency to make its appearance. But it seldom remains more than a few days at a time in that state of approaching firmness, the circumference of the firmness by degress subsiding, till all the continuous parts yield a feeling of softness on examining them, which, by a cursory examination, might be pronounced sufficiently natural. In the centre, however, may soon be felt something of a similar firmness, greatly reduced in size, of irregular dimensions, and figure; and this eventually diminishing, and as it were melting into a characteristic shape, can be ascertained, in the course of time, as a permanent, insulated, and small but not hard tumour; settled in the spot precisely where, by pressing gently with the finger at the commencement of the disease, it may often be discovered to differ from the parts around it, in being tender or slightly painful.

This, then, is about the time or period I have been disposed to regard as marking the introduction of the second stage. How long the first stage may indeed continue cannot be exactly stated. The symptoms by which it is distinguished may continue for two, three, or six months; sometimes near to or beyond twelve months, during which, as will naturally be expected, they are liable, after subsiding, to be occasionally again roused by new occurrences.

The second stage having commenced, and the tumour being distinctly discovered, it is felt as a small conical or oval body, neither so detached, as to roll easily under the finger, and yet so loose as, in lateral directions, to be easily moveable. In these circumstances, it may be readily distinguished by a circumscribed pulpiness, and a kind of soft doughy elastic firmness, with a peculiar smoothness of circular and compressible exterior. It is altogether free from much preternatural heat; there is no inflammatory redness of the cuticle, and in the parts immediately around there is a dulness of pain, and often a notable obtuseness of feeling. Its uniform ovoid figur is a striking contrast to the generic character of encysted tumour, which is said to be irregular and always varying in its form.

While the form, the mobility, the uniformity, and the sensations of the tumour in this stage are peculiar, its situation also merits attention. In the usual description of encysted tumours one of their characteristics is, that, being formed in the cellular membrane, they are to be felt immediately underneath the common integuments; a description undoubtedly leading to the expectation of feeling them almost in direct contact with the epidermis. In the state of the tumour alluded to at present this is not to be expected. For although it may be traced as in the cellular texture, and consequently not so deep as ordinary mammary tumours, still it feels deeper and more distant than the description above would indicate. It is somewhat superficial, and is felt at the same time as if a little softness of substance intervened between it and the skin,

which moves over it with a certain degree of sliding motion, affording one more criterion by which, in connexion with the other circumstances already mentioned, the nature of the disease may not unfrequently be ascertained with a considerable degree of accuracy.

The circumstances which have been just related are considered as symptomatic of the second stage. But, like the former, it is impossible to define the period during which this stage continues. The actual growth or increase of its size may be scarcely perceptible for years, or circumstances may occur by which it is obvious in a few months. It deserves to be remembered, however, that at times there appears to be an enlargement when there is actually none. At these times there is an appearance of its having acquired an increased solidity of texture; and yet, some time afterwards, it will be found to have returned to its former dimensions, and to have resumed its characteristic of circumscribed softness and pulpy feeling, while the surrounding parts have again become softened. When a change is thus effected, it recovers its loose and detached state, so that, from being in adherence somewhat firmly to the integuments, it may be again freely moved by the finger as formerly. Alterations of this nature are induced by various external causes, and consequently the disposition to grow is accelerated or retarded according to the frequency of their recurrence; which at once must be seen to render its duration indeterminate. It happened in one instance that the temporary appearances cf increased disease came on a short time after a fall, even although the breast, so far as it could be ascertained, had at the time sustained no direct external injury.

It is now that the third stage is ushered in; not merely by the increasing bulk of the circular tumour, but by the progressive accumulation of fluids within the embryo cyst. But there may be some difficulty in acquiring a distinct knowledge of this latter circumstance, previously to the cyst attaining the diameter of half an inch, or three-quarters of an inch. The reason of that difficulty seems to be an irregular thickness of the encompassing coat; for sometimes it feels as if there were either an increased quantity or an increased bulk of soft substance intervening between it and the finger, while at other times that intervening bulk is not perceived, which irregularity renders the sensations communicated by the contents very different on different examinations. This tendency to the thickening of the parts, however, gradually disappears; and hence, in the course of time, the accumulating fluid can be satisfactorily observed as well as the slenderness of the cyst's structure.

The state of the tumour as to the actual accumulation having become sufficiently manifest, it might be supposed that the future augmentation of bulk would be rapid and regular; but this is not always the case. It often proceeds as slowly and as irregularly in this respect as we have noticed it to have done in all its previous conditions. But the irregularity of this process is much more remarkable before the tumour attains a certain magnitude than afterwards,—an observation of great practical moment; for in proportion to its bulkiness, so is its tendency to rapid accumulation and consequent distention of the integuments. This circumstance was strikingly verified in one of my cases, from there being two tumours of the same nature in the superior portion of the same breast. The one tumour was about an inch distant from the other, without any communication, so far as various attempts to ascertain that point could be trusted. The one was also much smaller, being much later in appearing than the other; which afforded opportunities of acquiring information both as to that circumstance and as to several others of considerable importance.

The only important change in the character of the tumour, then, after the third stage has fully commenced, (waiving those considerations which relate to the effects of distention, either in causing a varicose appearance of cutaneous veins, or other local morbid impressions,) is the distressing bulk the tumour may attain from the increased quantity of sanguineous or fluid contents.

On the formation of the cyst little is accurately known.—The opinions of Bichat (Anatomie Générale) who refers the formation of encysted tumours generally to laws analogous to those which regulate the growth of organized parts, appear, if literally applied, scarcely compatible with the circumstances of the formation of the membranous envelope in question, which is the product, not of salutary but of morbid processes. But, not to enter into the various discussions upon this point, it seems probable that the cyst under consideration is not a new production in the part, but originally formed from extravasated fluids, collecting in one or more cells of the cellular membrane, along with an increasing tendency to accumulation. If this be the case, it seems not unreasonable to believe, that, with the increase of collected fluids, there may be a progressive dilatation and extension of its coats, which, by deriving accessions of compacted tissue from the adjacent cells and those in close contact, may thus acquire whatever is necessary for extension and strength. The objection that the neighbouring cellular membrane does not disappear or diminish in every instance, whilst the sac acquires a large bulk, is, if the animal operations during disease be duly considered, of very little importance.

The second consideration relative to the cyst regards its texture; and here it is proper to mention, that in the cases I have at one time or other had the opportunity of examining carefully, at an advanced period of the third stage, the cyst was almost invariably slender, with little vascularity, and little or no attachment to the surrounding parts. The uniformity of slender texture was not alike perceivable, however, on external examination, at every period of this stage. Sometimes it felt as if a little thicker, tenser, and

firmer than it did at other times; and yet this circumstance may be easily accounted for, by remembering what has been already stated;—namely, that the disease commences with a swelling, moderate, yet more or less extended; and that during the first and second stages symptoms of temporary disturbance of the vascular functions are not unfrequent, resembling chronic inflammation, and appearing in great measure to be confined to the cellular tissue, although varying both in the force of its activity and in the sphere of its operation. When the tendency to these affections is roused in this third stage by an incident unusual, yet so light as almost to pass unnoticed, the vascularity is re-excited, the parts affected, and the texture for a short time altered, as above remarked.

The third or remaining particular is the hemorrhagic process—that defect of integrity of the vessels in consequence of which the sanguineous congestion in the cyst is effected. In speaking of this, however, it is necessary to remember, that what is to be attempted with a view to illustration will refer to the third stage of the tumour principally and during the latter periods of tumefaction. If it is not likely that the effusion proceeds immediately from arteries, it must be admitted to issue from veins. Let us, therefore, see if this opinion be sanctioned by any thing like demonstration from practical experience.

In the first place, then, it will be allowed that veins may be so diseased as not only to cause an essential derangement of their functions, but also an obvious and very considerable dilatation of their coats or caliber. To account for this it is presumed none will be disposed to object to the belief, that, in some instances at least, the dilatation ensues from the loss of that tone in the tunics or walls which is necessary to their healthy condition; and it will scarcely be affirmed that in all instances the varicose state of veins is brought on solely by the loss of the necessary support of the adjacent parts, however possible it is that such a loss must add to the general effect.

The possibility of the loss of tone being therefore admitted, we may more easily account for the effusion of bloody matter within the sac, on the principle of open mouths of venous branches, which are more diseased at one time than at another, and more or less at these times in a weakened and relaxed state.

That the effusion may partly depend upon an increased *vis a tergo*, brought on by an increase of disease, is not unlikely; yet the possibility of additional causes removes not the possibility of the main cause, namely, the deficiency of healthy tone in those vascular branches which are immediately concerned with the diseased parts.

This may be farther explained by an observation which any one in such cases may make, and which I have had occasion to make in several instances. When, by means of a small aperture, made by introducing an instrument similar to the couching needle, I was under the necessity of emptying the sac of its contents repeatedly, it frequently happened, if the patient confined herself at these times to bed for a few days, or avoided the usual bodily exertions for a little afterwards, that the discharge, on soon again introducing the same instrument, had become much thinner, having less and less of the red matter of blood, and more serum, varying in proportion to the degree of rest.

So much was this the case, that the discharge at times was altogether without the colouring part, and not unfrequently near the transparency of limpid water; and even although it was limpid one day, the alteration the day following might be striking, from the patient in the interval taking a short airing in a carriage; the red matter being not merely augmented in quantity, but the fluid greatly more consistent and thickened, the breast at the same time having invariably less or more the appearance of new disease. Hence it seems perfectly obvious, whether venous trunks, branches, or capillaries be said to open into the sac, that the diameter of their caliber is influenced by morbific impressions, permitting them to pour out the grosser or finer parts of the circulating fluid; and hence also such occurrences seem to afford a pretty sure mark, that none of the vessels thus affected, under the like circumstances, can be very large.

It is not improper to mention, that, in Mr. Briggs' case already referred to, which, in consequence of Scarpa's observations on the same disease, was published in 1822, a large venous trunk was exposed during the operation for extirpating the breast, immediately below the edge of the pectoral muscle. It bled profusely, and was considered as the source from which the tumour had originated, although the mode of communication could not be demonstrated by an opening in the sac. In one of the cases given above, nothing worth notice could be traced on carefully inspecting the amputated breast, except a few scattered bodies, not unlike pretty large beads, which, from their limpid appearance, have been generally denominated incipient hydatids.

I should now proceed to investigate the most rational and proper methods of treatment applicable to this disorder. From the statements already made several practical inferences naturally result. On these it is unnecessary to dwell long; and I shall, therefore, conclude with a few brief remarks.

In the first place, it is obvious, that, previously to proposing any plan of local treatment, the stage of the disease ought distinctly to be determined. The stages differ so materially from each other in certain particulars, that what may be proposed for one, may be wholly inapplicable to another. A moderate degree of topical pressure by proper bandaging proves highly beneficial, for instance, in the second and third stages, but less so in the first stage. The repeated evacuation of the contents, when the tumour is very bulky, and the sac is much distended, in order to prepare

for other modes of treatment, is in great measure only applicable to certain periods of the third stage. The appropriation of medicinal means to the nature and circumstances of each stage tends to simplify the choice of remedies, and become a matter of no small importance.

Farther, if by piercing occasionally at an advanced period of the disease, the contraction of the sac is not only encouraged, and the growth arrested, but the integuments prevented from assuming that morbid disposition which excessive and increasing distention is sure to induce, while the subjacent parts are relieved from the pressure by which morbid operations are excited, little argument is required to prove the salutary effects from piercing as early in the third stage as circumstances permit. By doing so, the probability of success by vinous or other injections is greatly strengthened.

Again, the deleterious effects of incisions in the third stage, or openings of the sac with a lancet, and the safety with which, along with measures of subsequent precaution, the sac may repeatedly be pierced by a puncturing instrument, as described in the cases, it is hoped, are facts satisfactorily established.

Still farther, when it is considered that the disease originates in the vascular, but not the glandular part of the breast, many difficulties in the course of treatment will speedily vanish. To extirpate the embryo cyst or sac about the termination of the second stage would be a process attended with very little difficulty; and to speak of amputating the gland in advanced cases generally is almost superfluous; for, as the disease advances, the glandular substance seems at last disposed to disappear spontaneously.

To conclude. As a bad state of general health tends to influence every local disease, it is indispensable to use every means of improving the former. Sedulous management of the process of digestion, which should be conducted in such a manner as to nourish without oppressing, is the great object to be kept in view.

From the London Medical Gazette.

MEMOIR ON THE OBSTACLES PRESENTED TO DELIVERY BY THE MALFORMATION OF THE FŒTUS.*

By A. Dugès, Professor to the Faculty of Medicine, Montpellier.

We shall speak, first, of the obstacles presented by excess of size of the whole or part of the fœtus--as hydrocephalus, dropsy, &c.; and, secondly, of the difficulties resulting from the addition to the body of the child of some part of another fœtus, or the partial union of twins.

Excess of Volume.

There can be no doubt but that the size of the child, when considerable, may render the labour more slow and painful, particularly if the passage is but little dilated, and not sufficiently supple, as in a first confinement; or if the pelvis be rather narrow; and still more if to these be added an unfavourable position. But, independent of these accessory circumstances, it may be stated, that a large size of the child, provided its body be well proportioned, is never an entire bar to the spontaneous completion of labour. It is difficult, indeed, for a fœtus to exceed certain limits in its growth: either the uterus would resist a distention greater than it receives at the full period, under ordinary circumstances, and then the child would perish from the pressure; or else this organ, incapable of sustaining the expansion produced by the preternatural dimensions of its contents, would open and expel them. Children are said to have been born measuring 23 or even 25 inches from the vertex to the heels. These dimensions, however, have, no doubt, been made by guess, and as approximations: the last, indeed, would equal the stature of a child a year old. The general length is 18 inches, and the extreme would appear to be 22. I have seen an infant born of this last dimensions, and, next day, another a little less: the latter being 20 inches; it was plump, and weighed nine pounds and a half—the first born weighed about a pound more. Twenty-two inches from the vertex to the heels, then, may be stated as the extreme size of a well-proportioned fœtus; and it is easy to prove that the head of such a one will not exceed the dimensions of an ordinary pelvis. In fact, we know that the head of the fœtus, in passing the superior isthmus, always, in natural cases, performs an evolution which brings into relation with one of the oblique diameters of the isthmus its *occipito-bregmatic* diameter, which would not, even in the case we suppose, exceed four inches, or rather less. Now this is six lines under what is generally assigned to the part it has to pass through. The opposite oblique diameter is there presented to the bi-parietal, which is about the same length as the other. The occipito-frontal is not really presented to the abdominal isthmus of the pelvis, except in imperfect positions; and the same remark applies *â fortiori* to the occipito-mental diameter. These alone can present powerful obstacles to spontaneous labour: the former, indeed, is about five, and the latter five and a half inches. These unfavourable diameters may present in labours where the feet have come down, and when ill-directed efforts have been made by pulling to facilitate the delivery. The natural efforts alone would scarcely produce this inconvenience, as M. Desormeaux has shown, because they would produce an evolution analogous to that which takes place in the presentation of the vertex. This was completely proved in the case above-mentioned, where the fœtus measured 22 inches: the limbs and trunk were easily extracted, and efforts made to accomplish the delivery by pulling—but without avail. On leaving the pa-

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* Memoires de l'Academie Royale de Médicine.

tient for some time without assistance, the head was spontaneously expelled.

It is principally when we are obliged to turn an infant of large stature that great difficulty is experienced; and it is then that redoubled care is necessary, to avoid suffering the arm to cross the neck—to turn the face first towards one side of the pelvis, then towards the sacrum, and to depress the skin in such a manner as to render the sub-occipito-bregmatic, and the bi-parietal diameters, alone parallel to those of the narrow parts of the pelvis, and to the external organs.

I do not speak of the other indications which may present themselves, in the application of the forceps, &c.—the difficulties arising here, from the disproportion between the head of the child and the pelvis of the mother: it is evident that the precepts are the same as for the first degree of narrowness of the pelvis. I merely wish to speak of the diagnosis.

Of all the means which may lead to the discovery of a fœtus being larger than natural, none is either certain or easily applied; and none, therefore, is unequivocal, except the expulsion of one of the members before the rest of the body. The size of the abdomen after the escape of the waters, the uniform nature of the tumour it presented before, contrasted with the inequalities to be felt through the parietes of the uterus and abdomen—such are the marks which will tend to distinguish the case in question: first, from the distention produced by the waters; and, secondly, from the existence of twins, which give to the abdominal tumour a *bilobed* form, and in which we hear the heart beating in two different parts of the womb.

To these data we ought to endeavour to add the measurement of the part which presents—of the head, for example. Various contrivances have been suggested for this purpose, the accuracy of which I doubt. The simplest instrument is the finger, introduced per vaginam; but how deceitful is this method to an inexperienced practitioner! He who is only accustomed to judge of the dimensions of the head by sight, cannot be persuaded but that one, the surface of which he feels in the pelvis of the mother, is immense. Practice easily dissipates this illusion; and a finger accustomed to it is the best gauge of the size both of the head and the parts it has to pass through. It cannot only be passed along the former, but it can compare it with the circumference of the upper isthmus—judge how much it fills of this aperture, in what degree it presses upon its parietes, &c.; and it is always the *relative* proportion on which depend the practical results. In these investigations it must not be forgot, first, that the tumefaction of the integuments of the cranium often increase its volume as to height; secondly, that this tumefaction, as it were strangulated by the orifice, or by the arch of the pubes, always constitutes a portion of a much smaller sphere than the entire head; thirdly, that, in the first period of the labour, the head, not yet moulded to the parts, presents all the extent of its upper or vertical oval; fourthly, that, at a more advanced period, it is the occiput which becomes more particularly accessible to the finger. By overlooking these circumstances one would be led to think the head larger than it really is, in the first and third case, and smaller in the second and fourth.

The above remarks also apply entirely to excess of size, limited to the head, without any real disease of that part. Thrombus, to a considerable extent, beneath the skin of the cranium, sometimes deserves attention: less, however, on account of the increased size of the head, than from the deformity which it produces interfering with rotation; for example, when the tumour is engaged under the arch of the pubes, and becomes, to a certain extent, moulded to the parts. It would be still more difficult with the infiltrations which take place while the integuments of the head are putrid; and this circumstance only deserves notice on account of the great size which the distended integument sometimes attains. It might then, indeed, give rise to the idea that hydrocephalus existed; from which, however, it may be distinguished by its softness; by the fœtid discharge from the uterus; by the facility with which it accommodates itself to the dimensions of the passage which it traverses, &c. There can be no doubt but that this has constituted the majority of the cases of *external* hydrocephalus mentioned by the older writers.

Case of Voluminous Head—Prolapsus of the Cord—Turning.

F. Mathe, aged 41, arrived at the full period of her second pregnancy, without any other inconvenience than considerable constraint in walking. She was brought to the Maternité at midnight. The os uteri was almost completely diluted, and perfectly soft; the vertex presented in the first position, and a portion of the umbilical cord, retaining its pulsation, floated in the vagina. The waters continued to come away at intervals. To obviate the danger resulting from the compression of the umbilical cord, recourse was had, without delay, to turning. This operation was began without difficulty, in the usual way, and the extraction was easy until the head came into opposition with the superior isthmus, but it was then arrested by an unforeseen obstacle: in vain were gentle efforts made by laying hold of the shoulders and lower jaw; already the application of the forceps was in agitation, when a pain, aided by gentle pulling, perhaps better directed than before, produced the expulsion of the head, the great size of which afforded some explanation of the difficulty which had been experienced. This head was exactly five inches from the front to the occiput, and four across the temples; yet the child did not weigh altogether more than seven pounds and a half. It only lived a quarter of an hour. As to the mother, the placenta had scarcely come away when she began to complain of acute pain in the loins, which, in-

creasing, became fixed in the sacroiliac symphysis, and afterwards in the symphysis pubis. On examination per vaginam, it was found that a separation of the bones had taken place at this last, to the extent of two or three lines. Local and general antiphlogistic remedies, such as leeches, cataplasms, baths, and venesection, diminished these symptoms by degrees; at the end of a month the patient walked, but the convalescence was very slow, although it at last ended in complete recovery.

Water in the Head.

The only objects connected with water in the head, which we have to consider, are those which influence parturition; its frequency at the time of birth; the signs by which it may be known; its effects on labour; and the manner of affording the necessary assistance.

As to its degree of frequency, in consulting the registers left by Mad. Lacapelle, I find that, of 43,555 labours, which took place at the Maternité, between 1799 and 1820, only 15 cases of hydrocephalus at birth are mentioned, giving a proportion of 1 in 2904. We must, therefore, expect to meet with them very seldom in civil practice, and take care not to suspect their existence on vague grounds.

Among the signs, there are some which may be called conjectural—such as certain circumstances which may be looked upon as giving rise to hydrocephalus: for example, serous infiltration of the cellular membrane in the mother, during pregnancy, or a very large quantity of liquor amnii. We may also mention an hereditary disposition—for instance, if the woman has already given birth to one or more children labouring under this kind of dropsy; if she has herself a large and prominent forehead; if she is of a lymphatic temperament, and disposed to anasarca; we may then apprehend that all her offspring will be hydrocephalic. These, however, after all, are merely conjectural; and it is to the sensible signs—those afforded by the touch, that we must trust.

A surface which is large and little convex—which covers all the points of the superior isthmus, without, however, passing into it—a consistence which varies at different points, but which always presents resistance during the pains—softness, or even fluctuation, produced by the finger during the intervals: these are what is first perceived. By proceeding regularly, portions of the bones may be felt separated by membranous interstices, and the fontanelle—sometimes as large as the palm of the hand. If any other part than the vertex has presented, so that the head is only accessible at its base, the separation of the bones will be much less, but still will be easily appreciable. Such is the description of an hydrocephalus which is considerable; and the same marks measured by a smaller scale will also detect an instance of the affection proportionally less; but the head being then more convex, will also be less soft, and will protrude more into the pelvis.

We have pointed out in the preceding part the characters of an infiltration external to the cranium: we may mention, with regard to sanguineous infiltrations, that clammy softness which retains the impression of the finger, and which will be sufficient to prevent any risk of these being confounded with hydrocephalus. The head of a healthy fœtus is possessed, even when large, of a density of the bones, and narrowness of the fontanelles, which admits not of mistake. Sometimes a softness of the parietals is met with, which might give rise to error: this softness depends upon imperfect ossification of their inner and posterior part—there they are often very thin, pierced by spaces not yet ossified, and easily broken even by the process of labour: they yield to pressure with a crepitation like that of dry parchment, and spring up again in the same way. This last character is pathognomonic: once, however, I saw this region of the skull absolutely membranous, to the extent of an inch and a half in every direction; but the neighbouring bones did not yield to pressure in the same way as those of an hydrocephalic child, and the sutures had their accustomed arrangement.

I shall here also mention another source of error. It is an unnatural direction of the fœtus, in which the trunk is directed towards the loins of the mother, and the head rests above the pubes. This constitutes the *posterior obliquity*, denied by Baudelocque and others. The axis of the fœtus, far from being parallel to that of the superior isthmus, crosses it at an acute angle, and the head rests above, propped upon the anterior part of its circumference. The elevation of the head, and its immobility, notwithstanding the uterine contractions, and the natural dimensions of the pelvis, may the more readily lead to deception, as we cannot reach the vertex with the point of the finger without difficulty, and can scarcely measure its size by the ordinary process of the touch. But only to speak of the most important signs—this very elevation, and the hollow which remains between the head and the sacro-vertebral angle (which is easily discovered,) are quite sufficient for the diagnosis.

The indications do not depend merely upon the size of the head, which we never can ascertain with precision—they must also be guided by its flexibility—the disposition it evinces to enter the pelvis. A head of moderate size, soft and flexible, a vigorous mother, and contractile uterus, are circumstances which would lead the practitioner to trust to the spontaneous termination of the labour; but if the head advance slowly—if the woman be weak and exhausted—the forceps may be employed with advantage. The branches applied to the sides of the pelvis must be brought together with caution, and the accoucheur must pull very gently, lest he should produce laceration, or have the instrument lose its hold. If the infant presents the shoulder, and the head be disengaged, and appears of middling size, turning is indicated. The extraction of the head, if the child be living, may be assist-

ed by introducing the fingers into the mouth, and even by the application of the forceps: if it be dead, this will be known, because the trunk will have been already extracted, and we can then act without reserve, perforating the cranium, or applying the sharp crotchet; but if the ordinary perforator cannot be introduced either by the fontanelle or the occipital foramen, the *terebellum* (which I have proposed in cases of deformed pelvis) will pierce through the bone itself.

If the death of the fœtus could be ascertained with equal certainty when the vertex presented, the perforation of the cranium would still be applicable; but is it the same where the vertex presents, or the child has been extracted as far as the shoulders, and there exists a certainty, or at least a strong probability, of the contrary, the head being such that neither the forceps nor hand can effect its extraction? The hydrocephalic patient, it is said, will perish a few moments after its birth; it may, therefore, be destroyed to save the mother. But even in admitting this supposition, and considering the infant as the destroyer of the mother, does it rest with us to take away its life? We may, I think, be permitted to doubt this.

After it is punctured, the head often passes on from the mere efforts of the uterus; but, if assistance be required, it may be derived from the forceps, turning, or the blunt crotchet; but these manipulations do not come within the scope of the present discussion.

A young and robust woman gave birth, on the 23d November, 1819, at the full period of her third pregnancy, to an infant which was dead and hydrocephalic. This disease had been ascertained during the labour; but as the head made progress, although slowly, it was not deemed necessary to have recourse to any operation. It was not till twenty-four hours after the commencement of the pains that the delivery was completed. The mother did well. The child weighed altogether seven pounds ten ounces; the serum contained in the head rather more than thirty ounces. It was reddish and turbid, contained as usual within the ventricles, the parietes of which were very much attenuated. The head had the following dimensions: occipito-mental diameter, 6 inches, ten lines; occipito-frontal, 6 inches, 8 lines; bi-parietal, 4 inches, 11 lines.* I need scarcely remark that a head so voluminous as the above could not have been spontaneously delivered except from the assistance afforded by its flexibility.

A woman of strong constitution, aged 24, had rather a distressing pregnancy. Labour commenced on the 3d of March, 1824, and the membranes ruptured at five o'clock in the evening. The head remained above the superior strait, although the dilatation was complete. The pains ceased soon after, and, not having returned at eleven next morning, Madame Legrand directed one of her assistants to turn the child and terminate the delivery. On introducing the hand, she found the face of the child to the left and behind. She then laid hold of the left foot, and being unable to find the other, contented herself with proceeding methodically with the one she had reached. The delivery was readily effected of all the fœtus, excepting the head. This, however, resisted every effort: the forceps slipt over it, the blunt crotchet was of no avail, and it was soon perceived that the child no longer exhibited any signs of life. In an hour after, M. Dubois made fresh attempts to effect the delivery, with the same instruments, but in vain. He then took a sharp crotchet, and pierced the left side of the head, near the mastoid fontanelle: immediately a serous fluid escaped in abundance; the head was extracted, and it was discovered that it had been distended by a dropsical effusion. This case affords a striking illustration of the disposition which some women have to give birth to hydrocephalic children—both the instances above related having occurred in the same individual.

* French admeasurement.

Dropsy of other Parts of the Body.

Hydrocephalus does not always distend the whole head equally, but sometimes forms irregular tumours, which, however, on account of their softness, rarely present any obstacle to delivery. Ascites, still more uncommon than hydrocephalus, and water in the chest, yet rarer than either, do not necessarily prevent the child from being delivered either spontaneously, as I once saw, or with a little assistance. Indeed dropsical children are generally born before the full period. The infant above alluded to was born at the eighth month; one mentioned by Ramsbotham at seven months; and another by Portal, at the same period. A very large quantity of liquor amnii, and ascites on the part of the mother, may be causes of, and consequently conjectural signs of dropsy of the fœtus; but the truth cannot be ascertained in a satisfactory manner, until the expulsion of some part of the body has taken place: retained then by the enlargement of the abdomen, or thorax, it is arrested, and the accoucheur finds the pelvis filled by a large, soft, fluctuating tumour, which is easily evacuated by puncturing it with a trocar. The delivery will not fail to take place without difficulty, and unassisted by the operations recommended by various writers.

I shall only add one word with regard to those harder steatomatous tumours which may interrupt the progress of labour. They will often yield to pulling; and thus to remove them, if possible, or to empty them, if they contain a fluid, but always with the greatest possible tenderness towards the infant, if yet alive, are the only general directions which can be given.

Multiplication of Parts in the Fœtus.

Baudelocque has justly observed that it is extremely difficult to recognise the true state of matters under such circumstances. We

shall, therefore, only give in this paragraph, some of the signs by which the presence of twins may be distinguished from that of a double fœtus. Before the labour, the division of the belly into two lobes, the movements felt by the mother in two very different places; the beating of two hearts, heard at a great distance from each other, by means of the stethoscope, but with some variety in the situation; these are the marks rather of twins than a double child—unless, indeed, as in the case related by Walter, the uterus contains both twins and a double monster. If, when the labour has begun, we perceive two membranous bags, and the waters come away at two different times, the presence of twins may be looked upon as certain, for there are never two distinct envelopes for a double monster, and very seldom are natural twins contained in one. If one or both feet come down with the head, and if they are extracted by gentle pulling, without the head having a tendency to ascend, then we may be sure that there are two separate children; for a monster is never formed of two individuals, so placed as to have the head of the one by the feet of the other. But if several members present at once, it is only by carrying the hand into the uterus that it is possible to ascertain whether the individuals to whom they belong be joined together or separate.

The facility with which the natural efforts, either alone, or assisted even by persons of little skill, effect the delivery of monsters of the most disadvantageous formation, with regard to the mechanism of the parts, has always excited the astonishment of accoucheurs. The chief impediment is presented by the existence of two heads; and we shall briefly consider the cases where, along with this, the trunk also is double, and those in which it is single. If the two heads come down first, can the delivery be spontaneously effected? I think not, unless they are either very small, or have little consistence. It may be accomplished, however, if two fœtuses are loosely united, so as not to be always exactly parallel, but to present the parts successively instead of simultaneously. The direction of the fœtus, according to the axis of the superior isthmus, causes the head which is situated anteriorly to be likewise the inferior, and it is engaged in the pelvis while the other is kept back by the sacro-vertebral angle. The first head, as it advances, may be followed by the second, if small and soft, and the delivery be thus accomplished. But if the heads are both large, the second will, as it were, turn over the sacro-vertebral angle, and thus oppose the delivery.

It does not, however, happen thus if the feet or buttocks present; then the trunk, whether single or double, is expelled; after this the head, which is placed posteriorly, being the lower, (in consequence of the direction of the fœtus, which is then parallel to the inferior isthmus) becomes first engaged, and is afterwards followed by the other, the whole process being unattended with difficulty.

As to the monsters united by the vertex or occiput, they would afford no real difficulty unless the two heads presented at once. In this case, if the adhesions were sufficiently loose, they would follow the same course as in the preceding instance, but if the feet of one presented, the other would follow without difficulty.

As to monsters united by the breech, the point of union is never sufficiently flexible to admit of a double presentation, the two trunks being connected in a direct line, so that they can only advance by one of the heads, and the birth is then effected without difficulty.

From the Lancet.

ON SINGLE VISION, AND THE UNION OF THE OPTIC NERVES.

There is no subject which has more engaged the attention of philosophical men, than the phenomenon of single vision with two eyes. How does it happen that man, being provided with two eyes, has, nevertheless, unity of vision?

Dr. Wollaston believes that the faculty of single vision is attributable to a semi-decussation of the optic nerves; namely, that the contiguous half of each optic nerve on reaching the sella turcica, and there uniting with its fellow, does cross, and ultimately serve to furnish the retina to the nasal side of the opposite eye; the retina of the temporal side of each eye being formed by the expansion of half of the corresponding nerve. This distribution of the nerves has not been proved by anatomical demonstration; but Dr. Wollaston considers it established by induction, from the symptoms of disease, in some instances, which he relates.* It occurred twice to Dr. Wollaston himself, that he was not able to see but on one side of the axis of vision. The first time, the left side of each eye was affected, and he saw but the half of a man's face, or of any object he looked at. The affection did not last long; but several years afterwards, the same phenomenon occurred with the right eye. Two cases of a similar nature, are also recorded by Dr. Wollaston. Desmoulins† mentions a case in which this affection of vision occurred three times; the first two times objects, situated to the right of the axis of vision, were invisible; the third time, objects were seen on the right only of this axis.

Mr. Twining has a very ingenious paper on the foregoing subject, in the second volume of the Transactions of the Medical and Physical Society of Calcutta; and he denies, *in toto*, the accuracy of Dr. Wollaston's premises and conclusions. The following anatomical observations, respecting the structure of the optic nerves and thalami, and the effects of disease on those parts, he thinks, sufficiently establish the fact, that no decussation, or semi-decussation of the optic nerves, exists in the human subject.

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* Philos. Trans. 1824.

† Vide Elliotson's Blumenbach, p. 261.

Observation 1.—Mrs. Scott had a fungus of the left eye, for which the eye was extirpated. Several months afterwards, the patient died; and, on dissection, the left optic nerve was found to be of inky blackness, and this dark colour extended backwards from the orbit, far beyond the point where the nerves join. The diseased nerve, within the cranium, was as thick as the little finger, and the corresponding thalamus was about a third larger than the opposite one, but of natural structure. The dark colour above mentioned was confined to the left side. On the right side, the optic nerve was of its natural size and colour, and was merely attached to the black diseased nerve of the opposite side by cellular shreds, where the nerves come in contact on the *sella turcica*.

This patient had never observed any affection of the eye, until two years before the operation, when the morbid changes commenced; and, in the course of four months, she became gradually blind of the left eye.*

Observation 2.—Morgagni states that Hildanus had dissected a subject that had been blind of one eye, and he found the corresponding optic nerve wasted, even beyond the usual union of the nerves on the *sella turcica*.

Observation 3.—A man was afflicted with paralysis of the left side of the body; he was completely blind of the left eye, and the lids of both eyes were closed. The man died, and, on dissection, an ounce of coagulated blood was found in the right optic thalamus, extending into the lateral ventricle. Here was an injury beyond the junction of the optic nerves, producing blindness of one eye, not half blindness of both eyes, which it might be expected to do, if the semi-decussation of the optic nerves did exist.—*See Sir E. Home's Attempt to ascertain the Functions of certain Parts of the Brain.*

Observation 4.—A patient was affected with paralysis of the right side of the body. Dissection discovered erosion of the right thalamus. *Hemiopia* not noticed in this case.—*See M. Bayle on Paralytic Affections of the same side of the Body, with Organic Lesions.*

Observation 5.—A patient had hemiplegia of the right side, and lived four years after the first attack. On dissection, an effusion of blood was found in the thalamus. Hemiopia not observed in this case.

Rostan mentions in his work, *Sur le Ramollissement du Cerveau*, that the disease, when deeply seated, most frequently affects the corpora striata and thalamus of the right side. He states that imperfection of sight and blindness are frequent symptoms in that disease, and sometimes one pupil is more dilated than the other.

Observation 6.—Cæsalpinus says, "repertus est aliquando in anatome, alter ex nervis visoriis attenuatus, alter plenus; visus autem erat imbecillis in oculo ad quem nervus extenuatus ferebatur."

Observation 7.—Vesalius relates the dissection of the brain and optic nerves of a woman, who had, from a very early age, been blind of the right eye, the left eye being perfectly sound. The whole of the right nerve was attenuated in this case, whilst the left was sound.

Observation 8.—Morgagni states that Vesalius had observed the optic nerves to remain separate throughout their whole course, in a man who had always very strong sight.

Observation 9.—Mr. Cheselden relates the case of a gentleman who had strabismus, with double vision, produced by a blow on the head. By degrees, the most familiar objects came to appear single again, and in time all objects did so, without any amendment of the distortion. This fact shows, that points of the retina, not originally endowed with the joint possession of the correspondence supposed to depend on peculiar distribution of the optic nerves in the retinas, may, by habit, acquire that correspondence.

Such are the evidences from morbid anatomy adduced by Mr. Twining, as subversive of the doctrine held by Dr. Wollaston, whilst, for further refutation, he appeals to the labours of Vicq-d'Azyr, Wenzel, Reil, and Haller, who, although they dissected and studied the structure of the brain with great assiduity, failed in demonstrating a decussation of the fibres of the optic nerve.

It appears from an extract given by Mr. Twining, from Harris's Posthumous Treatise on Optics, published in 1775, that the sentiments of Sir Isaac Newton on single vision, were precisely in accordance with those advanced by Dr. Wollaston; and that, in some points, there is almost a verbal accordance between the opinions of these great men.

From the Transactions of the Medico-Chirurgical Society of Edinburgh.

OBSERVATIONS ON THE EFFECTS OF THE SUN'S RAYS ON THE HUMAN BODY.

By John Davy, M.D., F.R.S., Physician to the Forces, Corresponding Member of the Medico-Chirurgical Society of Edinburgh.

It is known to every one, that exposure to the sun's rays renders the skin brown; but I am not aware that this well known effect has hitherto been investigated with any minuteness, if at all, either in relation to the manner in which it is produced, or the parts of the skin in which it takes place, or its exact cause, or its consequences.

In this communication I shall have the honour of submitting to the Medico-Chirurgical Society of Edinburgh, the observations which I have made, with the desire of elucidating these points.

1. *Of the Changes connected with the Discolouring Effect of the Sun's Rays.*

For the purpose of ascertaining these

* Burn's Surgical Anatomy of the Head and Neck.

changes, a portion of the back of the fore-arm, which had never before felt the sun's action, was exposed to bright sunshine, in Corfu, during an hour and a half, on the 29th July, 1826, in the middle of the day, when the thermometer was at 78° in the shade. At the end of that time the skin was slightly painful, red, and hot. On the 1st August, the erythema was nearly in the same state; during the night the redness of the skin had been brightened, and the sensation of pain increased. On the 2d, there was very little alteration; a thermometer applied to the inflamed part rose to 96°, or 1° higher than when applied to the adjoining skin. On the 3d, desquamation had commenced at the circumference; hence, where the cuticle had separated, the part was brownish-red, and not painful. In the middle, where the cuticle firmly adhered, the colour continued to be rose-red, and the pain continued, though in a less degree. This middle part, it may be remarked, in which the erythema was most durable, was most inflamed, the sun's rays having struck on it perpendicularly; whilst, on the circumference, from the rotundity of the arm, they impinged on it obliquely. On the 5th, desquamation was making progress; pain had ceased; the part was reddish-brown at the edges, but still red at the centre; the temperature of the part was not above that of the adjoining skin. On the 8th, the part was uniformly of a reddish-brown; desquamation was still taking place, the new cuticle separating almost as fast as it formed, not in continuous pieces, as in the first instance, where the old was detached, but in small scales. On the 18th, the part was of a light-brown, with a very slight admixture of red, and its tendency to desquamation was very little greater than natural; in brief, it was in that state in which the skin is commonly said to be tanned by the sun.

2. *Of the part of the Skin in which the Discoloration takes place.*

Dr. Bostock, in his learned and useful "Elements of Physiology," remarks, "It has not been ascertained upon which part of the integuments the sun acts, whether upon the epidermis, the corpus mucosum, or the cutis;" and he immediately adds, "but it is probably upon the epidermis, because we are informed that the tan of the skin is removed by blisters."

Were it a fact that the skin is rendered fair by blisters, the argument would be plausible,—I had almost said conclusive; but, as it is well known that blisters themselves render the skin brown, this argument can hardly be received. From the observations which I have made, and from analogical reasoning, I am disposed to believe that the discoloration takes place beneath the cuticle, and that the seat of it principally is the surface of the cutis.

1st. As the sun's rays bleach hair, and as there is a considerable analogy between the hair and the epidermis, its effect on the latter, it might be expected, would be similar.*

2dly. I have carefully examined the cuticle detached in consequence of inflammation from insolation, and I have uot found it tanned in the slightest degree.

3dly. Are not the phenomena described in the preceding section, relative to the immediate effects and consequences of exposure to the sun's rays, almost sufficient to convince one that the cutis is the true seat of the discoloration? Were the epidermis the seat of it, it ought to be immediately discoloured by the sun's rays, which it is not; and when the epidermis separates, the skin should be fair; but the reverse of this is the case,—not till it separates does the skin lose its bright rose-red hue; and not till after several successive desquamations is the tan of the skin well impressed and established, and many months elapse before it disappears.†

4thly. I have examined, with some attention, the cuticle of the Negro, of people of colour, and of Europeans who have become dark brown from exposure to the sun's rays within the tropics. In each instance, when detached, it has appeared much less coloured than the skin; and, when minutely inspected, it has been found to owe its colour to colouring matter attached to it, detached from the cutis.

Lastly. I have preferred assigning the surface of the cutis as the seat of the discoloration, (supposing it to be proved that the cuticle is not,) rather than the rete mucosum or corpus mucosum of authors, as the very existence of such a texture is problematical. From the experiments which I have made on moles, and the coloured areola of the mamilla of fair persons, and on the skin of the Negro, I am disposed to believe that the colour, in all these instances, is owing to a colouring matter deposited in minute particles or filaments, on the surface of the cutis, as a secretion analogous, in its chemical properties, to the pigmentum nigrum of the eye.‡ In the skin of

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* Vide some remarks on this subject, contained in a paper on the Specific Gravity of different parts of the Human Body, which I have submitted to the Society.

† The discoloration produced in August, by exposure of the skin for an hour and a half, now, at the expiration of seventeen months, is just visible. I may add, that I have found it to continue much longer on a part always covered, as the arm, than on the back of the hand, which has been covered only in the open air.

‡ I find that the colouring matter of the skin of the Negro, and the pigmentum nigrum of the eye, are acted on very similarly by the three mineral acids, and a solution of potash, when heated, and by the sulphurous acid. By the former, both are dissolved; by the sulphurous acid, they are rendered of a light brown colour. They are not dissolved by these acids, or by the alkali, when cold; nor

the white, even in the parts discoloured, as the foresaid areola, I have not been able to discover any traces of a corpus mucosum, when the cuticle has been separated by means of immersion in the sulphurous acid. I have found the brown colouring matter, as I have already noticed, impregnating the surface of the cutis, and to be separated with difficulty by scraping it. In the case of the Negro, the colouring matter is deposited more thickly, and more in the form of a membrane; yet I have not been able to detach it as a membrane, and only in very minute portions, and that by scraping, when the cuticle has been raised and separated with as little inflammation as possible. The evidence in favour of the existence of a corpus mucosum, obtained either by maceration of the integuments, or by the application of blisters, appears to be very doubtful. By the first process, a gelatinous or mucus-like surface may be formed; by the second, a false membrane may be produced by the effusion of coagulable lymph, exactly resembling a corpus mucosum. I do not make these remarks hypothetically, but from experience,—from observing the effects of blisters on parts of the skin in which there have been moles; on the areola of the mamilla; and on the skin of the Negro. In all these examples, the effects generally are very similar. If the blister is mild, the cuticle is simply raised; in the instance of the Negro, with a very little colouring matter adhering to it. When severe and long continued, not only is the cuticle raised by serum effused, but also by coagulable lymph, to which is attached colouring matter, and which may easily be mistaken for a coloured rete mucosum, and which is easily separated as a continuous membrane. When severe inflammation and suppuration is excited, the colouring matter either comes away spontaneously, or is most easily detached.* It appears to be most firmly connected with the cutis in the instance of moles, next in that of the brown areola of the nipple, and least in that of the black skin of the Negro. The part, in healing, when covered with the first formed cuticle, is red; it soon becomes brownish, but a considerable time elapses before it acquires its former intensity of colour. In the instances of the Negro, in which I have watched its progress, the secretion of colouring matter began at the edges, and spread towards the centre; and then, after a few days, spots of black appeared in the middle, which enlarged till the whole area was coloured. When a part not discoloured is blistered, in healing, it passes from red to brown; and it is often a long time before the part regains its healthy hue; generally, I believe, the fairer the skin, the less it is made brown by a blister, and the sooner it recovers its original whiteness; and I believe the hotter the climate and season, so much the slower it regains it.

3. *Of the Cause of the Change of Colour, and of the Manner in which it operates.*

My experiments relative to the cause of the change of colour produced in the skin by the sun's rays, are not so satisfactory as I could wish. They tend, however, rather to prove that the effect is produced solely by the undecompounded rays. I have exposed, for more than two hours, and that repeatedly, the delicate skin of the under part of the fore-arm to the solar spectrum; and I have concentrated the differently coloured rays of the spectrum, by means of a lens on the skin, but without occasioning either erythema or discoloration.

Relative to the manner in which the effect is produced, is it immediate and direct; or mediate and indirect? In other words, is it the simple effect of the sun's rays impinging on the skin; or the effect of the inflammation which they occasion; or do the sun's rays act both ways?

That they act powerfully indirectly in producing discoloration, by exciting inflammation, the facts already mentioned, are, it appears to me, sufficient to prove. Indeed, whatever cause excites inflammation or irritation of the skin, seems to have an analogous effect, and to discolour it. Erysipelas, erythema, most of the exanthemata, burns, ulcers, excoriations, all occasion this effect (and, I believe, cold even is not an exception)* in different degrees, and very much in proportion to the intensity of the preceding inflammation, but whether exactly in that ratio it is difficult to determine. And we witness something of the same kind in mucous membranes. At least I have observed that the cicatrices of old ulcers of the intestines are always discoloured, and either gray, blue, or almost black, apparently according to the degree of severity of the local disease which they followed.

I have just said it is difficult to determine if the effect of discoloration be exactly proportioned to the inflammatory effect. There are circumstances in favour of its not being so. The erythema produced by strong acetic acid, and the vesication occasioned by the leaf of the common walnut tree, are followed by discoloration unusually dark and durable. Nor are there facts wanting which indicate that the change of colour may take place without inflammation, and go on increasing in intensity gradually, from continued exposure

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is their colour changed by a solution of chlorine in water,—contrary to what is commonly asserted of the colouring matter of the skin of the Negro. Both bear a high degree of temperature, apparently without change, viz. that nearly of a dull red heat.

* Thus it may be obtained in large pieces, very much resembling a membrane; but the connecting medium, I suspect, is coagulable lymph.

* Is not the dark colour of the inhabitants of the Arctic regions as much owing to the inflammatory or irritating effect of the extreme cold of winter, as to the scorching influence of the continued sunshine of summer?

to the sun, or even too bright light, without inflammation having been once produced. I remember an instance demonstrative of this, in the person of an excellent and most amiable young officer, (now no more,) a case of tubercular phthisis, complicated with other organic disease, who, in hope of deriving benefit from sailing and sea air, was taken from his room, where he had been confined many months, and conveyed on board ship, where he was placed under a convenient covering constructed on deck, sheltered always from the direct rays of the sun, but exposed to the bright light of the summer sky of the Mediterranean. In a short time, thus situated, he lost the pallid hue of the sick chamber, and became almost as deeply tanned as a native of southern Europe; and I was particular in ascertaining that the change had not been preceded by the slightest erythema, or any sensible desquamation. I may mention, in confirmation, the result of exposing, a second time, to the sun's rays the part tanned, as in the experiment first related.

On the 18th August, the part first acted on was exposed for two hours, between 10 and 12 o'clock, when the sky was unclouded, and the temperature, in the shade, about 80°. Immediately after this exposure, the tanned part was browner than before, and the adjoining white part, now exposed for the first time, was slightly red. On the 19th, the tanned part was distinctly browner and redder, a very little warmer than natural, and very slightly tender. The adjoining part was florid red, slightly painful, and hot. On the 23d, the tanned part was merely brown, a shade darker than before, while the adjoining part was undergoing desquamation, and beginning to lose its vivid inflammatory hue. And, farther, in confirmation, I may relate, that I have been at some pains to learn from natives of these islands, especially of the lower classes, who are very much in the open air, what effect they have experienced from the sun's action. The result of my inquiry is, that very few of them have ever experienced the blistering or scorching effect of the sun; and when they have experienced it, it has commonly been on a part of the body not accustomed to be exposed to light; and on some occasion of unusual exposure, as that of bathing in the open sea. From all which, may it not be inferred, that the sun acts both indirectly, by the medium of inflammation, in changing the colour of the skin, and directly, without the intervention of inflammation, in producing the same effect, or in heightening it when produced?

Lastly,—Of the consequences of Discoloration of the Skin.

Sir Everard Home has published an interesting paper in the Philosophical Transactions for 1821, in which he proves, that, when the skin is painted black, it is defended from the scorching effect of the sun's rays; and from whence he infers, that the dark rete mucosum of the Negro possesses the same protecting power.

I may remark, that I have made experiments similar to those of Sir Everard Home, and have modified them, and all of them with the same results. I may mention, that all the opaque colours which I have applied to the skin, whether red, orange, blue, or green, have afforded protection from the scorching influence of the sun's rays, equal to that afforded by black; and, I may notice cursorily, that the habit which the ancient Britons indulged in, of painting their bodies, may thus be referred to a purpose of utility, independent of show and ornament,—the paint with which they bedaubed themselves answering in part, in place of clothing.

But though I have confirmed the experimental results of Sir Everard Home, it appeared to me, when reflecting on the subject, that his inference was not so well established as at first view might be conceived. It is founded on analogy, and that analogy not perfect; as there is this difference between the skin of a white person painted, and of a negro with a black skin; that, in the one instance, the black surface is laid on the semitransparent cuticle, whilst, in the other, it is situated under the cuticle, and on the surface of the cutis. In the one instance, the extinguishing medium is external to the insensible covering of the body; in the other, it is in contact with the sensitive surface, and may be considered as part of it. Circumstances, too, relative to the very great penetrating power of the sun's rays, have had rather a similar tendency to augment my doubts of the strict accuracy of this analytical conclusion. As the facts which I now allude to, appear to me to be new and curious, I shall mention some of them. When the sun's rays are concentrated by a lens, they penetrate, I find, through bone, as a portion of the cranium;* through nine folds of black crape; and, what is most extraordinary, through rolled platinum. It was easy to ascertain their penetrating through the former substances, by a luminous point appearing on a surface beneath; but through the opaque platinum no light passed, yet the rays of heat passed, which was best indicated by the sensation produced, when the metal was placed on the sensitive skin, the only part of which affected was that corresponding to the focus of the lens, the metal itself not becoming sensibly warmer. Taking, then, into consideration the difference between the painted cuticle and the dark "rete mucosum," and this very remarkable penetrating power of the sun's rays, it appeared to me that more direct experiments than those of Sir Everard Home were requisite, to ascertain, beyond all doubt, if the function of the colouring matter of the skin of the Negro is really such as it has been inferred to be. With a view to this, I have subjected the skin of the negro to the direct rays of the sun, and I have made a similar trial on a mole on a

* This circumstance may help to explain the effect of the sun on the brain, in producing that malady commonly called *coup de soleil.*

fair skin. After two hours exposure to the sun, its rays moderately concentrated by a lens, (for the experiment was made in winter when the temperature was between 50° and 60°,) the part acted on, in which a dark brown mole was situated, became slightly red; the following day it was red, and just perceptibly painful; and about the fourth or fifth day, desquamation of the part commenced. The desquamation took place over the mole as well as the adjoining fair part, and the mole was evidently rendered of a darker colour. On the 27th December, when the sky was clear, and Fahrenheit's thermometer in the shade at 56°, a similar experiment was made on the fore-arm of a Negro, and continued the same time. The skin acted on was a little hotter than the rest, just perceptibly darker, and it felt, he said, slightly sore. On the following day, the part appeared to be very little darker, and he said that it was slightly painful and swollen, but this last mentioned effect was not perceptible to my eye. On the 31st December, the pain had ceased; there was not the slightest appearance of desquamation, and it was only just perceptibly darker than the adjoining skin.

From these results, I am disposed to infer, that the colouring matter of the skin of the Negro affords some protection from the scorching effects of the sun's rays, but not complete protection, and that were his skin as much disposed to inflame from the action of the sun's rays as the skin of the fairest European, this colouring matter would not prevent occasional vesication. Some of the facts already mentioned tend to support this opinion, especially the circumstance that exemption from the scorch-effect of the sun is not confined to the African Negro, but is enjoyed equally by all the various races of men,—the inhabitants of hot climates, who are much in the open air, and exposed to bright light rays, whether the colour be almost black, like that of the lower classes of Singalese, or of a dull straw colour, like that of the Bosjesman of Southern Africa, or of a ruddy brown, as in the instance of the Albanian shepherds of the mountains of Greece.

Nature, then, I may remark, is very provident, adapting the skin, impressed by the sun's rays, to bear them afterwards without inconvenience, or at least without painful suffering, the impression having apparently a protecting effect from farther annoyance, like the first attack of many of the infectious exanthemata; but with this difference, that the susceptibility to a renewal of the action is not long suspended, unless the cause is in constant activity. How long it is suspended is difficult to determine: it is suspended in different degrees, probably in persons of different complexions and temperaments, least in the fair, more in the brown European races, and most of all in the deeply coloured Asiatic and African tribes. Besides the foregoing, there are other means which nature employs to counteract the influence of the sun's rays on the human body, and to keep down animal heat within the bounds of health, in situations where otherwise it would be most apt to be in excess, and mount to a feverish height. These means seem to be of two kinds, one external, the other internal. Sweat constitutes one of the first; it not only cools the surface by evaporation, but by dispersing the sun's rays, it prevents their scorching effect, which is easily shown, by comparing the effect of the sun's rays, concentrated by a lens on a dry and wet skin. The internal means are not very obvious; but that they do exist, and often act beneficially, I have no doubt. Within the tropics, when on a journey, I have tried the temperature of my palenkeen bearers, just before setting out, when they were agreeably cool; and I have tried it again after three or four hours' exertion exposed to the sun, at a temperature between 80° and 90°; and I have found their temperature rather diminished than increased, as indicated by a thermometer, the bulb of which was placed under the tongue, or in the axilla.* Whatever occasioned this reduction, or keeping down of animal heat, this I allude to as the internal means of counteracting external heat. However it is explained (and it would not be difficult to offer a plausible explanation of it,) the fact seems to be curious; and if it is as new to others as it was novel to me, when I first made the observation, I need offer no apology for introducing it. Other circumstances in addition recur to my mind, when reflecting on this subject, by which nature fits man to bear with impunity, and with little inconvenience, the heat of the hottest regions of the globe of which he is a native. Within the tropics, the cuticle appears to become thinner, so as to confine the animal heat less, excepting on the sole of the foot and palm of the hand, when exposed unprotected to the action of a hot surface, or subjected to much pressure, when it becomes exceedingly thick. I remember observing, with surprise a Negro, lying with his feet so close to a fire, that the soles were heated to such a degree, as to be almost scalding to my touch, whilst to him they merely transmitted an agreeable warmth, propitious to the comfortable sleep in which he was indulging. In the inhabitants of the tropics, the exhalant arteries of the skin appear to be unusually expanded, and the whole apparatus peculiar to this texture unusually developed; and I believe the blood itself is less viscid, more fluid, and flows more freely through the

* Observations which I made many years ago within the tropics, with some care, indicated, that moderate exercise raised the temperature of the internal parts of the body, as well as of the surface; and that severe and long continued exertion rather had a contrary effect, as in the instance above recorded. Supposing it to be generally so, it will serve to account for the impunity with which a person having taken the first mentioned degree, may plunge into a cold bath, and the fatal effect (sometimes witnessed) of doing the same, when the exercise has been of the latter description.

cutaneous and subcutaneous vessels, so as to promote perspiration; by that means contributing to the cooling of the surface; and being cooled itself, it contributes again, when it flows back to the heart, to the reduction of the temperature of the internal parts. I deduce this opinion of the blood being more fluid at a comparatively high temperature as 88° or 98°, than at a low one, as 38° or 48°, from experiments on the blood, showing that a certain degree of cold thickens blood, and that a certain degree of heat renders it more liquid; so that in the one state it is better fitted for torpid hibernating animals, and in the other for animals in whom the functions of life are performed with energy. Moreover, the constitutions of the inhabitants of the tropics, and more especially of the Africans, are different from the constitutions of the fair races of the temperate zone. The African enjoys the best health, is in the highest spirits, and capable of the greatest exertions, in hot moist regions, where the temperature is seldom below 80°, and is almost entirely exempt from those fevers of the intermittent and remittent type, which have been, and probably always will be, the scourge and destruction of Europeans in hot climates.* But reverse the situation, and place the African where the European recovers his lost energy, shakes off the languor of the tropics, and is restored to health and strength, there the African droops, becomes languid, feeble, and diseased, and soon sinks into the grave. And thus it is, no doubt, that each race is propagated and multiplies in the situation most suitable to the development of its faculties and powers.

From the Transactions of the Medical and Chirurgical Society of London.

OBSERVATIONS ON DEPOSITIONS OF PUS AND LYMPH, *occurring in the lungs and other viscera, after injuries of different parts of the body.*

By Thomas Rose, Esq. M. A., Late of Balliol College, Oxford: Surgeon to St. George's Hospital.

It has long been known to pathologists and surgeons, that abscesses occasionally occur in some of the principal viscera of the thorax and abdomen in consequence of injuries of the head; and that, from the same cause, purulent effusions sometimes take place into the cavities of the pleura and peritoneum.

If we consult the writings of Morgagni, we shall find that so curious a fact did not escape the notice of that distinguished anatomist.† He tells us that Valsalva was induced, by his own observations, to say that the viscera of the thorax were sometimes affected in wounds of the head, and that he might have been so also by those of others, as Nicolaus Massa had, in 1553, met with apostemata in the thorax of a man who died delirious and paralytic, in consequence of a wound received upon the right side of his head, who had been known to be previously in good health, and not to have complained of pain in his breast, nor been troubled with cough, even after he lay ill of the wound: and as Marchetti, whose observations were better known than those of Massa, and were contained in the Sepulchretum, had often found the lungs and the pleura eroded after injuries of the head, and half the cavity of the thorax filled with pus, and had expressed his conviction that, in such cases, the matter descended from the head into the cavity of the thorax.

Morgagni further informs us, that there are some who have found pus in the belly, as the same Marchetti, who had even found a taint and purulent pustules in the spleen; that Bohn mentions the pleura, the lungs, and the spleen promiscuously, and that none of these omit the liver, which, by most others, is mentioned, as the only viscus into which pus can be carried after wounds of the head.

To show, however, that the latter opinion is erroneous, Morgagni states, that it never happened to him to see the liver thus affected, and that Valsalva in his numerous dissections only met with it once, and then matter was at the same time translated into the lungs also, and, in great quantities, into the cavity of the thorax itself. He refers us to Molinelli for a further confirmation, who had seen pus translated into different viscera, and not into the liver, and in some cases into the liver certainly, but just in the same manner from other wounded and ulcerated parts as from the head. According to Molinelli, however,* the viscera thus affected were always in the number of those contained in the belly.

Morgagni disproves by his own dissections, and by those of Valsalva, the notion that had been entertained by Marchetti, of the matter descending from the wound in the head into the cavity of the thorax. He illustrates his observations by four cases from Valsalva, in which the lungs were found diseased after wounds of the head. In one of these the patient, who was a young man, survived the accident more than two months. On dissection, it appeared that matter had formed under the dura mater, and that the lungs were hollowed out by various small abscesses. It is possible that, in this case, some previous disease may have existed in the lungs, and been brought to a more speedy termination by the mischief in the head; but the other three cases, which he refers to, were much more rapid in their progress, and in them the abscesses in the lungs obviously arose from the injuries which

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* In regions more unwholesome to Europeans than the Maremma of Italy, where not one European in a hundred escapes fever, and the majority attacked die, a case of intermittent or remittent fever occurring among Africans is rare indeed.

† Vide Morgagni on the Seats and Causes of Diseases, translated by Dr. Alexander, Vol. III. p. 100. et seq. Ed. Lond. 1769.

* Vide Morgagni, Vol. I. p. 786. Same Edition.

had preceded them,—one patient dying on the 14th, one on the 22d, and the third on the 25th day after the accident, and none of them having previously shown any tendency to disease of the chest.

In the first volume of the Memoirs of the French Academy of Surgery,* M. Quesnay, in a treatise on the operation of the trepan, gives a case of abscess in the liver, which followed a fracture of one of the parietal bones. The patient was conveyed to the Hôtel Dieu, and placed under the care of M. Boudon. On the tenth day after the accident he was perfectly tranquil, but he afterwards fell into a state of heavy and very disturbed sleep, accompanied by occasional rigours. It being supposed that effusion had taken place under the dura mater, M. Boudon, on the 14th day, removed two portions of the parietal bone with a trephine, and divided the dura mater, by which he gave exit to a spoonful of extravasated blood. After the operation, the rigours continued to recur, the patient complained of a sharp pain in the right hypochondrium, became comatose, and died on the 17th day from the time of the accident. It had been suspected, previous to his death, that a deposition of matter was forming in the liver, and, on a post-mortem examination, an abscess was found in the substance of the great lobe of that viscus.

This case is very analogous to one which occurred several years ago in a public hospital of this metropolis, where an eminent surgeon performed the operation of trepan under similar circumstances:—A dustman was brought into the hospital in consequence of having received a blow on the side of his head, which had detached a large flap of the scalp, and denuded a considerable portion of one of the parietal bones. The man was for a day or two extremely noisy and delirious, but these symptoms gradually left him, and he appeared, for a fortnight, to be recovering favourably; after that period, febrile symptoms came on, with violent rigours, which were followed by profuse sweats. The formation of pus being clearly indicated, it was judged advisable to remove a part of the parietal bone where the pericranium had been most detached. This was done, but the inner table of the bone was found adhering to the dura mater, which was perfectly healthy, and the operation of course afforded no relief. After the patient's death it was ascertained that a very large collection of matter had formed in the cavity of the pleura, and that the brain and its membranes were free from disease.

M. Bertrandi and M. Andouillé have each given a paper in the third volume of the memoirs already referred to,† expressly treating of abscesses in the liver formed in consequence of injuries of the head. The former of these authors attributes their formation to the obstruction to the return of blood into the right auricle, by the vena cava inferior, in consequence of the additional quantity which the cava superior has to bring back from the head. Passing over this theory, in the correctness of which M. Andouillé fully concurs, we shall find in the paper of Bertrandi some valuable information, and several very interesting cases of this disease. He remarks that authors, who have treated of these abscesses, have seldom been aware of their existence until they have discovered them in examinations made subsequent to the death of their patients; and he states that in his dissections he has often found such abscesses in those who have died of wounds of the head, where no suspicion whatever of their formation had been previously entertained. He further remarks that abscess of the liver from this cause is situated deep in the substance of that viscus.

If we refer to the valuable treatise on wounds of the head contained in the surgical works, or exposé of the doctrine and practice of Desault, published by Bichat, we shall find that Desault considered abscess of the liver to be one of the most common effects of injuries of the head; certainly much more common than is consistent with the experience of others. In speaking of the erysipelas which attends wounds of the scalp, he observes, "qu'il est rare que les symptomes deviennent violens, sans que le foie ne s'affecte, ou même qu'un dépôt ne s'y forme."

Desault regards these formations of matter as a consequence of the disturbance excited in the nervous system, and observes that they form a complication which in cases of concussion of the brain is almost inevitably fatal. Richerand, in his Nosographie Chirurgicale,* endeavours to rebut this theory, and to prove that these abscesses must depend upon some injury which the liver had sustained at the time of the accident. But this explanation is certainly erroneous, as it will be seen that they occur under circumstances where such a supposition cannot possibly be entertained.

It is curious that, whilst Desault represents abscesses of the liver as one of the most common consequences of severe wounds of the head, our countryman, Mr. Pott, who has treated so fully of the various effects resulting from these wounds, should have been entirely silent on the subject; and that the appearances which are presented by the viscera thus affected, should in like manner have escaped the notice of Dr. Baillie. Mr. Pott† has given one case in which a fatal peripneumony followed the operation of trepan, but he appears to have regarded that disease as an accidental occurrence, and not as a consequence of the mischief done to the head.

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* Mémoires de l'Académie Royale de Chirurgie, Tom. I. fol. 147. Paris, 1819.

† Vide Mémoires de l'Académ. Royale de Chirurgie, Tom. III., fol. 439 et 452.

* Vide Nosographie Chirurgicale, Tom. II. fol. 220. Paris, 1812.

† Chirurg. Works of Percivall Pott, Vol. I p. 127. London, 1779.

Mr. Samuel Cooper has expressed his suspicions,* "that the affection of the liver and primæ viæ (after injuries of the head) has been exaggerated by the French surgeons, since English surgeons, in their dissections, certainly do not find the liver frequently inflamed and suppurated in patients who have died of concussion."

But although abscesses of the liver, under such circumstances, may not by any means be so common as Desault would lead us to suppose, yet their occurrence in that, as well as in other viscera, after injuries of the head, seems to me to have been too little considered in the writings of English surgeons. Nor is it after injuries of the head alone, as the learned authors, whom I have hitherto quoted, would lead us to infer, that such abscesses are formed. They equally follow wounds of other parts of the body; and, during the Peninsular war, I met with several instances of their occurrence, in the lungs particularly, after amputations, and after other wounds of the extremities. I communicated these circumstances to Sir James M'Grigor in 1813, being then with our troops in Spain, requesting, that as the opportunities for observing the phenomena resulting from every description of injury to the body were at that time so extensive, he would inquire if similar affections of the lungs and different viscera had been observed by others.† I pointed out to Sir James as an excellent illustration of the disease in question, a case given by M. Larrey in the first volume of his "Mémoires de Chirurgie Militaire," three volumes of which interesting work I had then recently received, in which case abscess both in the liver and in the lungs followed amputation of the arm. The case‡ is that of General Caffarelli, and occurred during the occupation of Egypt by the French. The General died on the nineteenth day after he had undergone the operation, the wound from which was going on favourably, and on the sixth day after the attack of those febrile symptoms, which, as was ascertained after death, had indicated or led to the derangement of the internal organs. M. Larrey attributed the fatal result, and the disease of the viscera, to the effects of the Egyptian climate, with fatigue and other causes; and did not at all seem to suppose that they were connected with the previous wound or operation. But in the fourth volume of the same work, which was published at a subsequent period, that is after the peace of 1814, he gives§ an instance of a large abscess of the liver, in a Prussian soldier, occurring after a compound fracture of the arm. With a view to destroy in this case an artificial joint, M. Larrey introduced a seton between the fractured portions of bone, which, after a few days, was followed by enormous tumefaction and suppuration of the arm, and by an abscess in the liver which burst into the cavity of the abdomen. The period of the man's death is not stated. M. Larrey observes, that no doubt could be entertained of this abscess of the liver being attributable to the irritation and inflammation of the arm, as the man had not previously experienced any indisposition which could lead to the suspicion of his having hepatic disease. He gives in the same volume three other cases of abscess of the liver, following wounds of the head, and he states his opinion, that such abscesses are owing to the irritation excited in the liver by sympathy with the inflammatory action which had been established in the fibrous membranes of the cranium, or of the bones of the upper or lower extremity, but chiefly, he says, those of the same side, and by the metastasis to this viscus of the "miasmes ichoreux, ou d'un fluide plus ou moins acre et subtil." He adds that the communication of these morbid humours with the hepatic system, takes place more easily when they have not to cross the median line.

Mr. Hennen has also given three very interesting cases of the same nature in his work on the Principles of Military Surgery.* Two are cases of disease of the lungs, and one of disease of the liver, and all followed amputation. They do not tend to confirm M. Larrey's notion of the morbid humours not crossing the median line.

It appears therefore that the occurrence of abscesses in the viscera as a consequence of injuries of the head, more especially where these abscesses take place in the liver, has long been generally known, and that the circumstance of their following wounds of other parts of the body has of late been clearly pointed out, but the silence of the most distinguished pathological writers of this country respecting them, and the little notice which has been taken of their peculiar appearances, have induced me to think that the subject, although it has not any novelty to recommend it, might not be deemed entirely unworthy of the attention of this Society.

I have seen repeated instances of the disease in the lungs, in the liver, and in the spleen, and after various accidents. I have not been able to discover any peculiarity of constitution which could be regarded as predisposing to it. Many of the patients were young and healthy individuals, who, until the time when they met with the accidents, had never been affected with disease. Some of them were treated on the strictest antiphlogistic plan throughout, in consequence of the

* First Lines of the Practice of Surgery, Vol. I., p. 399. London, 1819.

† Sir James, in answer to this communication, informed me that Staff-surgeon Irwin had lost a patient of disease of the lungs following amputation of the thigh.

‡ Vide Mémoires de Chirurgie Militaire, et Campagnes de D. J. Larray, Tom. I., fol. 306. Paris, 1812.

§ Vide Mémoires, &c. de M. Larrey, Tom. IV., fol. 229. Paris, 1817.

* Vide Principles of Military Surgery, by J. Hennen, fol. 271. Lond. 1820.

nature of the accident they had experienced. In others (in compound fractures for instance,) as soon as the first inflammation had subsided, means were used for supporting the strength of the system. No difference as to the formation of the internal abscesses could be observed. In all the cases which I have seen, these abscesses took place at some period between the end of the second and that of the fifth week after the accident which gave rise to them.

The theories which ascribe their formation to injury done to the liver itself at the time of the accident, to obstruction to the entrance of the blood into the right auricle through the vena cava inferior, or to a direct communication for the transmission of matter from the head of the cavity of the thorax, are all obviously absurd. That of Desault, which attributes them to the disturbance of the nervous system, resulting from the injury, is probably the only explanation which can be given of their cause. They are to be classed amongst the effects of constitutional irritation arising from local injury, and are certainly striking illustrations of the irregular action in the vascular system to which that irritation may give rise. The attention of the members of our profession has lately been directed to this most important subject by the very valuable work of the President of this Society,* and it is to the principles which he has so ably illustrated that I should look for an explanation of the phenomena which I am now attempting to describe.

It is not very uncommon to find inflammation or congestion taking place in particular organs immediately after the constitution has rallied from a shock given to it by a severe accident or surgical operation, though that accident or operation be in a part of the body remote from these organs. In such cases the symptoms of inflammation are sufficiently marked; and should the disease proceed to a fatal termination, the appearances in the affected organ would, no doubt, correspond with those produced in it by inflammation or its consequences arising from any other cause. But the affections of the viscera, to which I have referred in this paper, have a peculiar character; and it appears to me that this may, in some degree, be accounted for by the rapidity wherewith, in the state of the constitution during which these abscesses occur, any congestion or inflammation, in whatever part it took place, would be followed by effusions of purulent fluid and of lymph. It is at the time when the parts, in which the injury took place, are in a state of suppuration; and in particular when, from the nature of these parts, or from the confinement of the matter, great irritation of the system has been for some time kept up, that such internal abscesses are apt to form; and it often happens, as is remarked by Bertrandi, that they have not been discovered until a post-mortem examination. But although constitutional disturbance, evidently referrible to an unfavourable state of the wound has, in all the cases which have come under my observation, preceded the formation of these visceral diseases, yet a favourable change has often taken place in the wound before the symptoms of the internal abscess have begun to manifest themselves; and we are sometimes able to detect the existence of the latter by the presence of rigours and other symptoms of suppurative fever at a time when the wound itself is disposed to heal.

The examination after death of those who have been affected with this disease, presents appearances which are well worthy of notice, though it is not easy to convey a correct idea of them in words. The disease consists, apparently, of depositions in the cellular texture of the affected organ, partly of a white or yellowish coloured lymph, and partly of pus. These depositions vary in size from beyond the bulk of the largest walnut to something less than a common pea. Where the lymph is most abundant, they may be described as a soft white tubercle of irregular shape, not contained in a cyst, but embedded in the cellular substance of the part, and gradually blending with its natural structure. When pressed, some pus exudes from them. Where the pus collects in greater quantity, it is lodged in an irregular cavity, probably in the middle of some of the tubercles, and the walls of the abscess are formed of flakes of lymph. The number of these tubercles and abscesses vary in different instances, there being sometimes only one or two, and sometimes the whole viscus being filled with them. In the lungs they are chiefly formed in the parts adjacent to the pleura pulmonalis, and there is often at the same time an effusion into the cavity of that membrane of a sero-purulent fluid mixed with lymph. In the liver and spleen they are dispersed throughout the substance, sometimes showing themselves in one or more yellowish patches, not elevated, on the convex surface of the great lobe of the former viscus, and at other times lodged in its substance. The parts adjacent to them show evident marks of increased vascularity.

I have said nothing of the treatment, and have little to suggest on that head. Our efforts must be directed, first, to subdue any excess of arterial action, and secondly, to quiet the disturbed state of the nervous system. When the abscesses are once formed, we shall find the truth of the observation of Desault, that they are almost invariably fatal.

I fear that I have prolonged this paper to much too great an extent, I shall therefore conclude by laying before the Society a short abstract of four cases of the disease, arising from injuries to different parts of the body.

Case I.—*Abscesses in the Lungs, with extravasation of lymph and pus into the cavities of*

* Vide An Inquiry concerning Constitutional Irritation, by Benj. Travers, Esq. F.R.S. Lond. 1826.

the pleura, after wound and amputation of the arm.

A soldier of the Coldstream guards, received a musket-shot wound in the elbow-joint of his left arm, at the storming of St. Sebastian's, on the 31st of August, 1813. The ball fractured both the condyles of the os brachii, and the coronoid process of the ulna. He was attacked with a considerable degree of irritative fever a few days after, but the inflammatory symptoms in the arm did not run particularly high.

After rather more than three weeks, these febrile symptoms continuing, with copious discharge from the wound, and his general health and strength declining, it appeared to me necessary to amputate his arm, and I was in hopes that the disturbance of his system would subside, when the only exciting cause of it, which I could discover, was removed. This was done on the 24th of September. On the second morning after, he appeared cheerful, and the febrile symptoms had diminished; but towards the middle of that day, he was seized with a slight rigour, which lasted for ten minutes or a quarter of an hour, and was succeeded by a most profuse sweat. The rigour returned on the evening of the 27th, and during that night, and through the whole of the 28th, the perspiration was constant. On the latter of these days, the stump was examined; union had taken place everywhere, except at the openings for the ligatures, and there was no tenderness in the part of the arm above it. He was ordered acid drinks, ripe fruit, and light nourishing diet. On the 29th, the same symptoms continued, with a dry shining tongue. All the ligatures came away, except that on the brachial artery. On the 30th, his breathing was found to be more hurried, but he took a full inspiration when desired, and it occasioned no pain. He had slept a good deal in the night, but his sleep was disturbed, and he moaned frequently. He still continued to perspire copiously. His bowels had from the first been perfectly regular. At this time the stump was flaccid, but union was going on. During that day the hurried breathing increased, and at four the next morning he expired; being the seventh day after the operation, and the thirty-first after he had received the wound.

I examined the body on the day he died. In the cavity of the thorax, on the left side, more than a pint of sero-purulent fluid was found effused, mixed with loose flakes of coagulable lymph. The pleura pulmonalis and pleura costalis were glued together in parts by the lymph, and were highly vascular. Numerous circumscribed abscesses were found imbedded in the cellular structure of the lungs; principally in those parts of them which are nearest to the pleura. These abscesses were perfectly distinct from the parenchymatous substance of the lung, by which they were surrounded, and which appeared in no way affected, except by showing higher vascularity. They did not appear to be invested by any cyst of condensed membrane; and in many of them instead of pus, or mixed with pus, was a whitish substance, probably common lymph. On the right side of the thorax, the appearances were somewhat similar, but the effusion was to a much less extent. The viscera of the abdomen were healthy.

Case II.—*Abscesses in the Lungs, Liver, and Spleen, after compound fracture of the leg.*

William Deane, 21 years of age, was admitted under my care, into St. George's Hospital, on the 23d of July, 1825, with a compound fracture of the tibia and fibula of his right leg, occasioned by a load of gravel having fallen upon him.

On the 27th considerable tumefaction had come on in the limb, inflammation having diffused itself through its cellular tissue, and a good deal of bloody serum had begun to ooze from the wound. This was followed by a sharp attack of erysipelas, which spread over all the thigh, and over the principal part of the integuments of the abdomen. By free incisions wherever matter could be detected, venesection once or twice repeated in moderate quantity, and saline diaphoretics, these symptoms subsided favourably, but left him a good deal emaciated. On the evening of the 2d of August the erysipelas had disappeared; he had little fever, but profuse discharge, and he then began to take light nourishment with appetite.

On the morning of the 3d of August he was seized with a severe rigour, followed by sickness. His pulse at noon was 120; his tongue brown and dry; he had great heat of skin, and restlessness, and complained of a sense of uneasiness about the pit of his stomach. An aperient medicine was given him, and afterwards effervescing draughts, with small doses of antimonial wine.

On the 4th he was better, and his pulse had sunk to 108. There was a slight relapse of erysipelas over a part of the abdomen. The wound discharged profusely; but no matter was lodged, as there were free and depending openings.

On the 5th he was still better, his pulse was 96, and the erysipelas was again subsiding. Some wine was allowed him; and light nourishing but liquid food continued.

On the 6th his countenance was not so favourable. He was ordered sulphate of quinine. This produced no good effect, his tongue gradually becoming more parched and dry.

On the 9th, he complained of an unpleasant sense of rising from his stomach, with an excessive heat in his throat, but he could bear pressure on the abdomen without pain. In the evening of that day he had an attack of stupor, and lay for many hours in a state of nearly complete insensibility, with contracted pupils. He died on the evening of the 11th, being the twentieth day from the date of the accident.

The body was examined on the following

day. The vessels of the pia mater and brain were more turgid than natural, and there was a considerable effusion of serum into the ventricles. In the thorax, there were several circumscribed abscesses in the lungs on each side, but chiefly in those on the right. These were situated in the outer part of the lungs, towards the pleura, and varied in bulk, from that of a small pea to that of a large nut. Their contents were evidently a loose sort of lymph, through which pus was everywhere beginning to be diffused, as could be shown by its issuing when they were gently pressed. On the upper part of the convex surface of the great lobe of the liver a large mass of a similar character was visible, of a perfectly white colour, appearing under the peritoneal covering. It was two or three inches in diameter, and when cut into, was found to extend at least two inches in depth, into the substance of the liver, which everywhere bordering on it, had a natural appearance, and did not seem to be in any way condensed. A somewhat paler line marked where the two structures, that of the liver and of this mass, were blended. The mass consisted of loose lymph, with pus diffused through it, as in the lungs. On the right edge of the great lobe, under the short ribs, there was another mass of the same nature, but of a smaller size, and one or two similar patches under the capsule of the spleen. No attempt at union had taken place in the fracture.

CASE III.—*Abscesses in the Lungs, Liver, and Articulation of Clavicle and Sternum, with effusion into the Thorax, after a bruise and wound of the foot, and a fractured fibula.*

George Stacey, 18 years of age, and apparently of a healthy constitution, was admitted under my care into St. George's Hospital, on the 17th of July, 1827, in consequence of an accident from a cart-wheel having passed over the outside of his left foot. There was a small wound under the little toe, made apparently by some sharp substance, which had penetrated under the first phalanx, about an inch into the sole of his foot. Considerable ecchymosis had taken place over all his instep and foot, and there was a simple fracture of his left fibula two inches above the ankle. Leeches, cold lotions, and aperient medicines were ordered, and the limb was kept quiet, and supported on a pillow. The leeches were repeated several times.

On the 23d he had shiverings, after a restless night; and these were followed by diffused cellular inflammation over every part of the foot, and by erysipelas extending up the leg and thigh, with enlarged glands in the groin. The integuments in different parts of the foot were divided, to set the inflamed parts at liberty; and on free openings being obtained for matter which had formed under the fascia plantaris, the febrile disturbance began to subside.

On the 4th of August he was reported convalescent, and at his earnest request was ordered some meat for his dinner on the following day.

On the 5th he had a severe rigour, which lasted for more than an hour. A purgative medicine was ordered, and he was again put on light diet; and it is to be observed that the rigour came on before he had taken the meat.

On the 7th, the rigour returned at the same hour as on the 5th, and lasted about the same time. The limb continued perfectly quiet, all the wounds were healing, and no cause could be discerned for these febrile attacks. He had never had ague, but stated that where he had been working that disease prevailed. He was directed to take two grains of the sulphate of quinine every four hours.

The rigour returned again on the 8th, followed by much heat and a very quick pulse, and continued afterwards to recur at irregular intervals, being generally succeeded by profuse sweats.

On the 10th it was observed that he had slight ptosis of the upper eyelid of the right eye; his pulse was quick, nearly 150; his tongue dry; his countenance unfavourable, and with a yellowish tinge. There was no appearance of matter forming in any part of the leg, and he could bear pressure over the abdomen. In the evening some degree of emphysema and a little effusion of fluid were detected at the articulation of the right clavicle with the sternum. He had met with no accident in the part to account for this. On the evening of the 11th he died, being the 26th day after the accident.

The body was examined on the following day. In the head the arachnoid appeared more opaque than natural, and there was some lymph effused on the under surface of the anterior lobes of the cerebrum and round the junction of the optic nerves; matter was found effused into the cellular membrane over the sternal extremity of the right clavicle, and into the synovial cavities on each side of the inter-articular cartilage between that bone and the sternum.

The pleura on both sides of the thorax was very vascular, and distended with a considerable quantity of sero-purulent fluid mixed with loose flakes of lymph. This was more abundant on the left than on the right side of the chest.

The lungs on each side contained numerous small abscesses and soft tubercular masses, principally adjoining the surface of the pleura. These varied in size from that of a hazel-nut to less than that of a small pea; and in the middle of some of the tubercles there was an irregular cavity filled with pus. One small abscess was found in the substance of the great lobe of the liver, at some distance from its surface.

CASE IV.—*Abscesses of the Liver and Spleen, after fracture of the Skull, &c.*

A French gentleman, upwards of 30 years of age, was brought to St. George's Hospital on the evening of the 27th of July, 1825, and admitted under the care of Mr. Keate. He was in a state of complete insensibility, in con-

sequence of having fallen from his horse and pitched on the side of his head. He died on the 18th of August, the twenty-third day after the accident.

On examining the body, it was found that a fracture had taken place, commencing a little above the posterior and inferior angle of the left parietal bone, and extending across the occiput to the foramen magnum. There was a considerable quantity of blood extravasated at the base of the skull. The brain itself was ruptured at the lower part of the posterior lobe on the left side, and pus had formed at that part between it and the pia mater. Both the liver and spleen were studded over their surface, and throughout their substance, with soft tubercular masses consisting of lymph mixed with pus, and with circumscribed abscesses of different sizes.

The following very interesting cases will further illustrate the subject. They are communicated by Mr. Lawrence, who has allowed them to be annexed to this paper, a permission of which the author of it is happy to avail himself.

Cases communicated by Wm. Lawrence, Esq. F.R.S. Surgeon to St. Bartholomew's Hospital, &c.

Thomas Scarborough, æt. 33, was admitted into St. Bartholomew's Hospital on the 6th of January, 1827, in order to have a loose cartilage removed from the knee-joint. He had laboured under an inguinal hernia for six or eight years, and had been subject, during that time, to bowel complaints. He had a somewhat sallow and unhealthy look, and a whitish tongue, yet considered himself in good health, and had followed his ordinary occupation as a labourer, to the time of his admission, having experienced only temporary inconvenience from the complaint in the knee, which had existed about three years.

After some attention had been paid to the state of his health, the operation was performed on the 13th of January, and a perfectly smooth white piece of cartilage, with a small bony nucleus, was taken out of the joint, having been previously fixed on the external condyle of the femur.

17th.—The wound has united, without the slightest heat or swelling of the joint. In the evening, however, without any assignable cause, bleeding took place from the wound, and ceased spontaneously by the formation of a coagulum, which distended the incision. On the next day the sallow appearance of the countenance, which had been noticed at the time of admission, was much more conspicuous, and the conjunctivæ were quite yellow. During the following week, the joint, which was kept quite quiet, remained free from pain and swelling, and a thin fluid escaped from the wound. There was no fever.

After passing a restless night, he became very ill on the 26th, with heat of skin, thirst, loss of appetite, costiveness, white tongue, with a dry brown streak in the middle. The joint was swollen and painful, the margins of the wound inflamed, and a purulent fluid mixed with synovia flowed from it on pressure. (Twenty leeches to the joint; aperient, and afterwards saline medicines.) He continued in nearly the same state till the evening of the 29th, when the febrile symptoms increased, and he was bled to fourteen ounces, the blood being strongly cupped and buffed. In consequence of continued febrile disturbance, twenty ounces of blood, exhibiting the same character as before, were taken from the arm on the 31st.

He was better on the 1st of February. Twenty leeches to the knee-joint, which continues inflamed and swollen, and discharges much pus.

During the night of the 2d he was restless and delirious. From this time he continued to sink: paralysis of the right side, more particularly of the arm, was observed on the 5th; and he expired on the 7th.

Examination of the body twelve hours after death.—The knee-joint contained a small quantity of healthy pus: the synovial membrane was thickened, vascular, and in some places dark coloured. An abscess on the outside of the knee, containing two ounces of pus, communicated with the joint; and the skin was separated from the subjacent textures, in the neighbourhood of the wound, by a cavity like that of an abscess. A small portion of the upper and anterior part of the tibia was denuded; the cartilages were unchanged in all other parts. Several small, yellowish, elevated spots were observed on the convex surface of the liver, which was slightly agglutinated to the diaphragm by recent adhesion. On cutting into them, a thin purulent fluid escaped, leaving a yellow fibrous substance, very much like the flakes of a scrofulous abscess. In some of them the fibrous substance predominated; in others, the thin yellow pus. These depositions varied in size, from that of a pea to that of a hazel-nut. They existed in great numbers throughout the whole liver, but the thick edge was more particularly loaded with them. I counted thirty on the surface of one section; there must consequently have been many hundreds throughout the liver. The other abdominal viscera were healthy. The arachnoid membrane covering the hemispheres was partially elevated by serous effusion under it: this was particularly apparent over the posterior lobe. A small deposition of healthy yellow pus, about the size a horse-bean, was found at the side of one of the posterior convolutions of the left hemisphere.

H. A. Porter, 51 years of age, a corpulent man, addicted to drinking, was admitted into St. Bartholomew's Hospital the 19th of January, 1827, for an old ulcer of the leg, with much surrounding inflammation. He had a poultice to the ulcer, and was placed on milk diet; he was twice bled in the arm, and took opening medicine. Under this treatment the leg improved rapidly, and he felt altogether much better; but three days after the second bleeding the wound of the vein became pain-

ful, and was found to be slightly inflamed. (A bread poultice was applied to it.)

Jan. 30th.—Although the arm was easier after the application of the poultice, shivering fits came on last night, followed by heat and thirst; pulse 120. (Venesection to ten ounces: the flow of blood was arrested by syncope: the blood was not buffed. Twenty leeches to the arm. A dose of calomel and jalap. A saline draught every four hours, with antim. tart. gr. j. and potassæ nitr. ʒss.)

31st.—Inflammation has extended to the axilla, the arm being red, swoln, and painful on pressure, from the elbow to that part. (Thirty leeches to the arm, and a large blister afterwards.) A severe shivering fit was experienced this afternoon.

Feb. 1st.—Restless night from the blister; pulse 102, and small; tongue white; no appetite; bowels open. The antimony had been gradually reduced to a quarter of a grain in each dose, but it still caused so much sickness that it has been left off.

4th.—He has remained nearly in the same state, and has had several shivering fits. He complains to-day of severe pain in the left knee-joint, which is somewhat swollen. The arm is easy, and a small quantity of thin pus flows from the wound in the vein on pressure. The pulse hard, full, and 100; tongue white and dry; great thirst; bowels confined. (Senna mixture immediately; a saline draught every four hours, with tinct. digitalis ♏xij.)

5th.—He has passed a comfortable night. The left knee and thigh are greatly swollen and very painful. The joint is distended to the utmost with effused fluid, causing a large prominent tumefaction above and at the sides of the patella. All the superficial veins of the knee and thigh are excessively swollen, and form a very conspicuous net-work. The limb is slightly red, and preternaturally hot. Pulse 140, and soft; tongue white; bowels open. (Six doses of digitalis have been taken; let it be increased to ♏ xv: four grains of calomel every four hours: ʒxvj. of blood to be taken from the knee by cupping.)

7th.—He complained yesterday of pain in the right shoulder, which continues, without swelling or redness. The knee was relieved by the cupping, and is nearly free from pain, though the swelling is not much diminished. The arm is less painful; bowels very open. (The calomel to be left off; the digitalis continued.) In the evening he was more easy; the pulse small, hard, and 120; tongue brown and dry; great thirst.

He expired on the morning of the 8th, and the body was examined ten hours after death.

The cephalic vein, which had been punctured, was thickened, and contained pus for about two inches below, and four inches above the wound, where a coagulum of blood was found, filling the cavity. Above and below these points the vessel was healthy, and the other veins exhibited no morbid change. The abdominal and thoracic viscera were healthy. The arachnoid membrane was thickened, opaque, and whitish. The cellular texture of the pia mater was loaded with serum, and an increased quantity of fluid was found in the ventricles. The cavity of the knee-joint was filled with a tolerably thick pus, of an uniformly reddish colour, as if from an intimate admixture of blood. The synovial membrane was thickened, with an irregular and almost villous surface: it was extremely vascular in its whole extent. The cartilaginous coverings of the femur and tibia had undergone considerable absorption, so that the convexities of the femoral condyles and the corresponding excavations of the tibia were completely bare. The cellular substance covering the capsule of the knee, under the exterior muscles, was inflamed, thickened, and loaded with pus. This texture was in the same state on the surface, and throughout the whole substance of the vasti and cruralis muscles. Sections of these muscles presented a most singular appearance, their large fasciculi being separated apparently by layers of thick yellow pus. The matter, although precisely similar in colour and consistence to that produced by phlegmonous inflammation, was nowhere collected into an abscess, but was diffused through the cellular structure, as serum is in the case of anasarca. In the rest of the limb there was effusion of a bright light yellow serum. The cellular structure exterior to the orbicular ligament of the right shoulder was filled with thick yellow pus; but the cavity of the joint and the deltoid muscle were natural.

Captain L., 34 years of age, became the subject of calculus in India, and returned home to undergo the operation. He had feverish symptoms of intermittent character after his return; and I prescribed for him the effervescing saline draughts, under which the fever disappeared, and he also lost entirely the calculous symptoms. He was however still bent on undergoing the operation, which I performed for him, and removed a calculus of moderate size. It was necessary to bleed him largely from the arm on the evening of the third day. Soreness came on about the puncture in two days, and this was gradually followed by general swelling, and slight redness of the whole limb, with excessive pain and great feverishness. The local and general symptoms were not controlled by any of the measures adopted, and the case ended fatally at the end of the third week, symptoms of inflammation having come on on the same side of the chest as that on which he had been bled in the last forty-eight hours. The basilic vein was thickened by inflammation up to its termination, and the veins corresponding to it were in the same state down to the back of the hand. The coats of the vessels were red, and the surrounding tissue was indurated by inflammation. The interior of the inflamed veins was partially roughened, as if by the deposition of lymph: they contained pus throughout. The whole subcutaneous tissue of the arm was inflamed, and partially infiltrated with serum. The axillary vein and the continuation of the trunk to the heart were free from inflammation. The pleura was violently inflamed; the cavity

contained about a pint of whey-like fluid, mixed with pus and flakes of lymph.

A married woman, 25 years of age, who had been a great spirit drinker for some years, was bled in the left arm on account of an accident, and, pursuing her ordinary occupation, that of weaving, experienced a severe attack of inflammation in the vein and neighbouring part of the limb, for which she was received into St. Bartholomew's Hospital on the 2d of December, 1826, being the fifth day from the commencement of the inflammation. She died on the fourteenth, her symptoms at one time having been so much relieved that we entertained great hopes of her recovery. On the 7th the inflammation and swelling of the left arm were much diminished, and there was copious discharge from the puncture of the vein of thin matter, sometimes yellow, sometimes reddish. She now suffered very greatly from pains over the body, but more particularly in the extremities. She passed a very restless night from this cause, and suffered greatly the next day from pain in the calves of the legs. On the 11th she again suffered much from pains in the limbs. On the 12th it was found that matter had formed under the skin of the right arm, without redness, and five ounces of good pus were discharged by a puncture. At the same time a painful swelling of the left knee, from effusion into the cavity, was observed. We were not allowed to examine this part after death.

From the London Medical Gazette.

MEMOIR ON ANEURISMS *caused by Fractures and Gun-shot Wounds, and on their Treatment, according to the method of Ariel.*

By M. Dupuytren.

Amongst those serious accidents which are liable to accompany fractures and gun-shot wounds, the tearing of a principal artery and the consequent effusion of blood, presenting the characters of an aneurismal tumour, form a complication which not only compromises the safety of the limb but also the life of the patient; and which, according to the practice hitherto in use, presents no other resource than amputation, with all its risks and consequences. In reflecting on the frequency and variety of fractures and wounds, it is evident that these aneurisms must be very common; nevertheless, authors scarcely contain any examples of the kind, either owing to their being in reality more rare than might be supposed, or, what is more probable, because attention must be awakened especially to this point in order that our observation may be directed to phenomena otherwise sufficiently striking. M. Dupuytren has only found one instance of this complication in authors: it is reported by Petit, who, in a fracture of the tibia, without any external wound, perceiving a large ecchymosis spread over the whole leg and foot, whilst at the same time these parts became cold and of a dark colour, thought that the artery (probably the anterior tibial) was opened; he therefore made an incision which laid the vessel bare, and stopped the hemorrhage, but he does not say by what means. From this solitary example, which is defective in many important particulars, authors have generally repeated the fact of aneurism being an occasional complication of fracture or gun-shot wound, but without adducing any farther instances. After having observed that all writers agree in recommending amputation of the affected limb, M. Dupuytren relates three cases which occurred when Pelletan was surgeon in chief of the Hôtel Dieu. In the first there was a simple fracture of the left leg; a general swelling of the limb showed itself from the beginning, and continued to increase, but without any alteration of the colour of the skin. On the 26th day an incision was made in the centre of the swelling, and gave issue to some clots of blood at first, and afterwards to a jet of arterial blood, which was arrested by pressing the femoral artery; the thigh was then amputated, and the patient recovered; the source of the hemorrhage was not ascertained. In the second case the fracture was also in the left leg: up to the fifteenth day nothing particular had been remarked;—at that period the patient complained of pain in the calf of the leg; they persisted, and on the thirtieth day a tumefaction was perceived at the middle part of the leg, of a shining appearance and bluish colour. Soon afterwards all the characteristic marks of aneurism were perceived, and the swelling continuing to increase, the amputation of the thigh was performed in spite of the patient's weakness. Dissection of the limb showed an aneurismal pouch consecutive to the lesion of the peroneal artery, which had been torn by fragments of the fibula. The patient died of pneumonia forty-six days after the accident. In the third case there was also a fracture of the left leg, but accompanied by a wound, which gave issue every day to a greater or less quantity of blood: however, the consolidation of the fracture was completed the seventy-sixth day, when on a sudden a hemorrhage, attended with an enormous tumefaction of the leg, came on. The wound was enlarged, plugged, and the following day amputation above the knee was performed. The patient died the seventeenth day after the operation. Dissection of the limb showed the anterior tibial artery pierced by five or six openings, and the fractured bones united.

Thus in three cases amputation had only once succeeded, a circumstance that ought to induce practitioners to avoid it, more especially since not above a fourth of those who have suffered amputation of the principal members recover. It is matter of astonishment that in these cases ligature of the trunk of the wounded artery has not been resorted to; it was a case similar to the above that induced M. Dupuytren to depart from the ordinary routine, and to give the patient a chance of saving his limb.

Case.—On the 2d January, 1809, a woman, 62 years of age, made a slip in running along the street, fell, and fractured her left leg.

When brought to the Hôtel Dieu on the following day, M. Dupuytren, wishing to reduce the fracture, discovered in the calf of the leg a regular pulsation, sensible both to the touch and sight, isochronous with the contractions of the pulse, and which ceased when pressure was made on the femoral artery. These symptoms demonstrating the existence of an aneurism, caused without doubt by the rupture of one of the arteries by the fragments of the broken bone, M. Dupuytren thought that the ligature of the artery of the limb would be preferable to amputation. Independently of the cessation of the growth of the tumour, the ligature would prevent the necessity of exposing the seat of the fracture itself to inflammation and suppuration. In consequence of these reflections the femoral artery was tied in the middle of the thigh; the heat and sensibility of the limb were not for a moment interrupted. From the fifth day the tumour sensibly decreased; the ligature came away on the fifteenth day; the formation of the callus took place slowly, doubtless because the source of nutrition was in a great measure interrupted: it was scarcely formed at the end of the second month, but it was perfectly consolidated at the end of the fourth, when the patient quitted the hospital cured.

A similar instance was observed in 1815, by M. Delpech, who relates it in his Clinical Surgery. These two examples put the following principle beyond all doubt—viz. that the rupture of the arteries of a limb, caused by the fragments of a broken bone, may be cured by the ligature of the artery above the disease, even when this rupture is accompanied by an aneurismal tumour. Was it possible to conclude from the above cases, in which the skin was whole, that the same success might be obtained if the skin was torn, and the seat of the fracture in communication with the external air? It rests now to demonstrate that gun-shot wounds complicated with aneurism, do not require amputation more than fractures complicated with that accident, and that they may also be cured by the ligature of the principal artery of the limb. M. Dupuytren calls in the aid of facts to prove this position.

Case.—M. De Gombaut, Chef d'Escadron, received in February, 1818, a wound from a pistol ball, which passed through the upper part of the right leg, from before backwards, and from without inwards, passing between the tibia and fibula, which last it slightly injured. A very violent hemorrhage occurred at the time of the accident: a strong compression made upon both the wounds arrested it, and, assisted by the tourniquet applied to the thigh, no fresh hemorrhage outwards was perceived till the third day: from that time it was renewed at intervals, and the tumefaction of the limb, as well as the pulsation, continued augmenting more and more, and MM. Aumont and Depres, who attended the patient, called M. Dupuytren in consultation.

The foot and the leg were violet coloured, swollen, and cold. At the upper part of the leg there was a tumefaction, accompanied by tension, and a pulsation isochronous with those of the heart. Upon this tumour were seen two openings with unequal edges, closed within a few hours only by clots of blood, which each pulsation appeared to raise up and to threaten to detach. Every thing proved that the ball had pierced one or more arterial trunks; it was evidently impossible to tie the wounded vessels. Amputation appeared the readiest resource to MM. Aumont and Depres, but M. Dupuytren proposed the ligature of the femoral artery, there being a possibility of recurring afterwards to amputation if the condition of the patient became worse. The operation was immediately performed, and had the happiest results. The ligature came away on the twentieth day. During this time the wound of the leg discharged the blood little by little; some portions of the clothing and pieces of bone were brought away by the suppuration, and three months after the accident M. Gombaut walked as well as ever.—*Archives Générales, July.*

From the Lancet.

NICOLAS CHERVIN'S RESEARCHES ON THE NATURE OF YELLOW FEVER.

This distinguished individual has been engaged, during nearly the whole of his life, in the study of this formidable disease; neither dangers nor pecuniary sacrifices could change his intention to visit almost every part of America where this calamity reigns. He did not return, until after ten years of incessant study and incredible toil, to his native country, to reap the fruits of his admirable zeal. By the following concise account of his travels, we intend to call the attention of our readers to the work of M. Chervin, which is shortly expected to appear.

In the year 1814, he left Paris for Guadaloupe, which he reached in December of the same year. Before he began his journey, he had eagerly studied all French, Italian, English, and Spanish works on the subject, and from them he was disposed to believe in the contagious nature of the yellow fever; but he endeavoured to make his own observations, free of all prejudices, in order to arrive at a clear result. During the year 1815, he had no opportunity of observing the disease, but in 1816 and 1817, he met with it very frequently, under the most varied circumstances; he then began to doubt the correctness of his former opinion. He then went to Martinique, Antigua, St. Christophe, St. Martin, St. Thomas, and Porto Rico. In August, 1817, he arrived at St. Domingo, where the yellow fever just happened to rage in its most malignant form. Having made many observations there, he went to Jamaica, thence to Cuba, and Port-au-Prince. He adapted his route always as much as possible to the course of the disease. During his absence from Jamaica, the fever had made terrible ravages amongst two newly-arrived regiments; on his return, it was still very violent,

and afforded him the best opportunity of examining the most important circumstances with regard to its contagious or non-contagious nature.

In November he went to Havanna, where he remained till the 12th of February, 1820. At New Orleans he arrived at the period when the fever generally appears; the epidemy was terrible; he witnessed it during six months, and then left for Savannah, being informed that the disease raged there with an unprecedented malignity. He was, however, disappointed; by neglect of the captain, his ship proceeded at once to Charleston, where he was very well received. He remained but a short time, and went to Savannah, notwithstanding the most anxious representations of his friends, and in spite of the information that no less than six physicians of that place had fallen a sacrifice to the fever. When he arrived, in October, the rage of the epidemy had by no means subsided, and he found an ample field for observation. He was, indeed, so deeply engaged in his studies, that he forgot to write to his friends of New Orleans, who were so certain of his death, that in the Medical Society of that town a funeral oration was read to his memory. They were soon, however, agreeably surprised at the news, that M. Chervin, after staying two months at Savannah, had pursued his journey towards the north. He visited North Carolina, Virginia, Alexandria, Georgetown, Washington, Baltimore, Philadelphia, and New York. In the beginning of 1822, he left Boston for Guadaloupe, and having visited Paramaribo, Cayenne, Demerara, Barbadoes, &c. he sailed from Martinique to Spain, where he arrived in February, 1823.

Notwithstanding the dangers which he must have anticipated from the political events in that country, he travelled over the whole peninsula, and came to Cadiz shortly before the commencement of the siege by the French army. After a long stay in that town, he made excursions into the provinces of Malaga and Barcelona, and, at last, in 1824, returned to Paris. Here dangers of a new sort awaited him; he was calumniated, and the most ridiculous political charges were brought against him, but he gloriously triumphed over the envy of his enemies, and exposed their base intentions.

The number of documents which M. Chervin has collected exceeds 800; they consist, mostly, in authentic statements of the physicians and the magistrates of those towns and districts which he visited, and which are subject to the epidemy. Besides the valuable results of the most ample experience as to its treatment, they contain most important materials for deciding the question of its contagious or non-contagious nature.

Chervin has made more than *five hundred* post-mortem examinations. He has often *swallowed* some of the *black fluid* found in the stomach of the deceased; he *rubbed the whole surface of his body with it, and always remained free from infection.*

The following are the general results of his inquiries:—Of more than 500 competent practitioners, only 48 are in favour of the contagiousness of the yellow fever, 483 being decidedly against it. In those parts of America where it most frequently rages, nobody believes in contagion; the extension of the disease seems entirely owing to the atmospheric constitution, and to local causes; the latter consist, partly, in putrid effluvia; there exists, in no case, a clear proof of contagion having taken place, and all assertions to the contrary are founded either on false testimonies, on defective observations, or on erroneous inferences from correct observations.

When the yellow fever raged epidemically in Catalonia, five French physicians, Mazet, Pariset, François, Audouard, and Bally, were ordered to go to Barcelona, and a Cordon Sanitaire was established by the French army along the Spanish frontiers; the disease was declared highly contagious, and the work of Bally, François, and Pariset,* tended to confirm this opinion. Chervin being convinced that the measures against the extension of the fever were entirely useless, presented a petition to the Chamber of Deputies in 1825, and afterwards asked for a special committee to examine his documents, and then to decide upon the necessity of the Cordon Sanitaire. Eighteen of the most eminent physicians were elected for this inquiry, and it was not until the 15th of May, 1827, Coutanceau read to the Academie de Medécine the general conclusion of the committee:—"That the documents of M. Chervin contain decisive proofs against the contagiousness of the yellow fever, and, consequently, against the necessity of the Cordon Sanitaire." This report was instantly ordered to be printed, notwithstanding Pariset's opposition; after two days, however, this order was retracted, to give the French physicians, who had been sent to Barcelona, sufficient time for their defence. At the end of 1827, Coutanceau's report, with Pariset's reply to it, appeared; and Chervin himself published a small treatise on the transactions of the committee. He is, at present, engaged with the edition of a large work in four quarto volumes, of 600 pages each, with maps and plans. Till the publication of this important work, we suspend our further remarks on the subject, and refer those of our readers, who wish for a detailed account of the discussions in the Académie de Medécine, to the numbers of the Revue Medicale, from May to October, 1827.

From the Transactions of the Medical and Chirurgical Society of London.

THE CATARRHUS ÆSTIVUS, OR SUMMER CATARRH. By J. Bostock, M.D. F.R.S., &c.

In the tenth volume of the Society's Transactions there is an account of a disease which

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* Histoire medicale de la Fievre jaune observeé au Espagne, particulierement en Catalogne dans l'annèe, 1821, Paris, 1823.

I conceive to be of a specific nature, and which from its symptoms, and from its occurring only at a certain period of the year, I propose to name the catarrhus æstivus. In my former communication, I detailed the symptoms as they occurred in my own person; my present object is to extend my remarks to the affection as it occurs in other individuals, to inquire into its cause, and to make some observations on the mode of treatment.

The number of cases which I have either seen, or of which I have received a distinct account, amounts to eighteen, besides about ten others, which are less correctly ascertained. They all agree in the complaint making its appearance at the same season of the year, in its seat being the membrane lining the nose, the fauces, and the vesicles of the lungs, and, for the most part, in the paroxysms being excited and the symptoms aggravated by the same causes.

One of the most remarkable circumstances respecting this complaint is its not having been noticed as a specific affection, until within the last ten or twelve years. Except a single observation of Heberden's,* I have not met with any thing that can be supposed to refer to it in any author, ancient or modern. I have at various times stated the particulars of my case to some of the most eminent physicians in London, Edinburgh, and Liverpool, and have very gratefully to acknowledge their kindest sympathy and attention; but, until very lately, it was always considered by them as an anomalous train of symptoms, and no one appeared to have witnessed any occurrence of a similar kind, and the same sentiment I recollect to have prevailed in this Society nine years ago, on the reading of my former paper. The first intimation which I received of a contrary opinion, was from the late Dr. Baillie, who, in the summer of 1822, related to me three cases which he considered as similar to my own. Yet, as there appears to be nothing, either in the cause or nature of the complaint, which can induce us to suppose that it is actually a new disease, we are obliged to conclude that it had been regarded as a mere modification of the common catarrh.

The twenty-eight cases referred to above, all agree in the complaint commencing about the end of May or the beginning of June, and continuing from four to eight weeks. Most of them are attended with fulness of the head, stoppage of the nose, sneezing, watering of the eyes, and discharge from the nostrils. In about half of the whole number the respiration is considerably affected, and in three or four instances it is almost the only symptom. Some of the cases are attended with distinct cough, most of them with irritation of the fauces, and some with a degree of sore throat. Actual inflammation of the eyes is not a very common occurrence, and in some of the cases there is not even the discharge of tears, or the irritation of the eyes. The degree of general indisposition varies very much in the different cases; in some, the patient, during the whole period, is unable to use any exertion, or to continue his ordinary occupations, while, in other instances, he feels no inconvenience, except what arises from the fits of sneezing, and the copious discharge from the nose.

I have not been able to trace any decided connexion between the peculiar symptoms and any circumstance of age, sex, constitution, or mode of life in the patient. For the most part, indeed, I have found, that in very young persons, the first symptoms that are observed are sneezing and running of the eyes, that the chest is not affected until a later period of life, and that, as age advances, the purely catarrhal symptoms decrease, while the pectoral symptoms have a tendency to increase. With respect to age, I have no account of the complaint commencing earlier than it did in myself, at about eight years, nor have I heard of any very old persons being affected with it; for the most part, however, it seems rather to increase with the advance of life than the contrary, and I have no account of any one who has been once affected by it, ever afterwards losing the tendency. It is remarkable, that all the cases are in the middle or upper classes of society, some indeed of high rank. I have made inquiry at the various dispensaries in London and elsewhere, and I have not heard of a single unequivocal case occurring among the poor. A considerable majority of the cases are males, but I have an account of some females, who suffer severely from the complaint. There is no decided evidence of the complaint being hereditary, except that there is an instance where three members of the same family are affected by it.

I have not been able to ascertain with any great degree of precision, whether any specific temperament is peculiarly subject to it. Those cases that have fallen under my own inspection have been generally of a spare habit and liable to stomach affections, but I have met with exceptions to this rule. It does not appear to be confined to any particular situation; it occurs alike in towns and in the country, and I have not heard of any districts, the inhabitants of which are peculiarly subject to it.

The immediate cause of the symptoms seems to be sufficiently obvious; it consists in an increased action of the vessels of the membrane which lines the eyelids, the nose, the fauces, and the pulmonary vesicles, by which it becomes acutely sensible to external impressions, has its natural secretions augmented, and probably its bulk increased; to this last cause I think we may ascribe the very distressing sense of dyspnœa which exists in some of the cases. Although this membrane is continued without interruption over the different organs that are the seat of the affec-

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* "I have known it (catarrh) return in four or five persons annually in the months of April, May, June, or July, and last a month, with great violence." This passage was pointed out to me by Dr. M. Hall: I am also indebted to Dr. Hall for the account of a well-marked case of the catarrhus æstivus.

tion, yet it is observed that the different parts are affected in different degrees. Hence we may divide the disease into four varieties, according as the eyes, the nose, the fauces, or the lungs is the part more immediately affected. It is in the last variety only, that I have observed the constitutional symptoms of fever and the subsequent debility to exist in any considerable degree; and in this case I think we may account for the effect, by supposing that the thickened state of the membrane which lines the vesicles, prevents the oxygen of the inspired air from duly acting on the blood.

With respect to what is termed the exciting cause of the disease, since the attention of the public has been turned to the subject, an idea has very generally prevailed, that it is produced by the effluvium from new hay, and it has hence obtained the popular name of the hay-fever. As it is extremely important to ascertain the truth of this opinion, I have made it the subject of distinct observation, as far as regards my own person, and by minutely attending to the accession of the symptoms, for a number of successive seasons, in relation to this supposed cause, I think myself fully warranted in asserting, that in my own case the effluvium from hay has no connexion with the disease. The following observations will, I think, be sufficient to prove this position.

In consequence of the benefit which I always experienced from fresh cool air, I made choice of Ramsgate as my residence during the summers of 1824, 1825, and 1826. The last two of these years will be long remembered for their excessive heat; but by procuring a house on the cliff, exposed to the German ocean, and commanding complete ventilation, by avoiding bodily exercise, and indeed seldom leaving the house until evening, during the year 1825 I nearly escaped the disease. In the year 1826, I have reason to believe that the disease was much mitigated by the comparative coolness of the situation, but still I had many decided and some severe paroxysms. Now it is well known, that there is not an acre of meadow ground in the whole of the Isle of Thanet, and in the year 1826, in consequence of the great drought, all the little patches of grass, which may be supposed to exist on road sides or elsewhere, were completely burnt up. Nor is this all; during many of the hottest days, the wind blew steadily from the south-east, so that the nearest land to windward of the house which I occupied, was on the French coast, a little to the north of Calais. Yet during this time, whenever I relaxed from my plan of discipline, and exposed myself to the sun's rays, or by any means quickened the circulation, the symptoms recurred in full force.

The last year, 1827, with the exception of a short period in July, was cold. I could not conveniently remove to any great distance from London, and I spent the summer at Kew. This situation might have been chosen for the purpose of the experiment, for almost the whole of that part of the country consists of hay-grass, which was cut while I was in the neighbourhood. In consequence of the coolness of the season I did not confine myself to the house, but walked out daily, occasionally in the Kew gardens, and was surrounded by many hundred acres of hay-grass in all its different states, yet except during the few hot days, when I suffered as usual, my complaint was in a much less degree than the average.

But although I think the evidence, as far as respects myself, to be quite decisive, I acknowledge that I have received accounts from various quarters, of individuals, who have felt no doubt that the complaint was brought on by the effluvium from hay, and was relieved or prevented by avoiding this effluvium. I will not venture to assert that this opinion is incorrect, but I believe that in most cases we may explain the facts more naturally by supposing, that the patients, at the time when they conceived themselves to be inhaling the effluvium from hay, were also exposed to heated air or sunshine, or had been using bodily exercise. Experience, however, must decide the question, and when the subject is once fairly brought into view, it will not be difficult to collect a sufficient number of facts to enable us to form our opinion.

With respect to the cure or mitigation of the complaint, I regret to say, that except in so far as we are able to avoid those circumstances which bring on the paroxysm, I have been able to obtain very little satisfactory evidence. Most of the patients have tried a change of residence, some from town to country, others from country to town, and some have removed to various parts of the island, or even to the continent. In two cases of considerable severity, the patients have felt convinced that they were better in London than in the country; in another case the patient conceived that he derived great advantage from exposure to sea air, but, in other instances, similar trials have not proved successful.

As far as regards medical treatment, an anxious desire to obtain relief from an annual indisposition of several weeks' continuance, and sometimes of considerable severity, has induced me to try, with the greatest perseverance, every remedy which held out the least prospect of advantage. I think myself warranted in asserting, that, on the whole, the depleting system is injurious, and that some benefit is gained by a moderate use of tonics. This is the only point in which the various accounts that I have received from others and my own experience appear to agree, and in general it would seem that the symptoms proceed nearly in the same way under very opposite plans of treatment, and are very little influenced by medicines of any description.

The experience of many years has taught me not to expect a cure for the complaint, so that I now only aim at relieving any peculiar urgent or distressing symptom. Bathing the eyes in tepid water, and fomenting the face generally, occasionally applying small blisters

to the chest, mild purgatives, small doses of ipecacuanha, Dover's powder, squills, and digitalis, bathing the feet in warm water, a moderate but not spare diet, perfect rest, and carefully avoiding all extremes of heat, comprise the whole of the means that I have found useful to myself. In order to prevent others from making useless experiments, I may remark, that among those things which I have tried without success are bark, iron, opium, mercury, large blisters, topical bleeding, the waters of Harrowgate and Leamington, the baths of Bath and Buxton, sea-bathing, the shower bath, abstinence from wine and animal food, and a more free use of them; each of these having been made, as it may be said, the subject of distinct experiment, and persevered in, until some circumstance rendered it necessary to discontinue them, or until they produced a decidedly injurious effect.

While this paper was in the press, I was informed by a friend, on whose accuracy I could place implicit confidence, that great relief had been experienced in two cases of the complaint, by applying to the eyes and nostrils a very weak infusion of tincture of opium, in the proportion of one or two drops of the tincture to an ounce of water. I regret to say, that in the trial which I have hitherto made, it does not appear to produce the same beneficial effect on my symptoms.

From the London Medical Gazette.

OBSERVATIONS ON CATARACT.

By M. Dupuytren.

M. Dupuytren has recently made some comparative trials of the two methods of operating for cataract; namely, by depression and extraction. Of these we shall take an opportunity in a future number of giving some account; at present we purpose laying before our readers some general observations on the subject, taken from the *Clinique des Hôpitaux.*

Before undertaking to operate for cataract, M. Dupuytren enjoins the minutest inquiry into the general state of the patient, with particular reference to any concomitant diseases. The conditions which he regards as frequently contra-indicating the operation, or at least pointing out the necessity of delay, are, rheumatism, pulmonary catarrh, and derangement of the stomach or bowels; constipation, hemorrhoids, shingles, and many other diseases, may, he thinks, give rise to mischief in the eye, already irritated by the operation. If, for example, rheumatism be present, the operation may occasion its metastasis to the head; the eye and its appendages then become painful, and ophthalmia is excited, which often proves extremely severe. Whether this phenomenon is to be attributed to the rheumatism or to irritation, is of little importance; the fact remains the same, that it is not prudent to operate in such cases, experience having shown the evils which result from so doing. It is necessary, then, in the first place to attack the rheumatism, and if it is determined to operate, whilst some degree of pain still continues, it is prudent to apply a blister to some part at a distance from the head. If pulmonary catarrh be present, besides the injurious effect of the cough on the circulation of the head, we should fear, if the operation of depression had been performed, lest the cataract should resume its former place in consequence of the succussions communicated to the head during the paroxysms of coughing. If there be any affection of the stomach, not only have we to dread the same mechanical inconveniences which result from the cough, and which in this case may be produced by vomiting; but, also all those complications, which must necessarily result from the sympathy between the stomach and the eyes, since there are some affections of these which depend entirely upon derangement of the digestive organs; and, moreover, if the operation has been performed during the existence of disease of the stomach, even although but slight, it is requisite always to place the patient during a longer period on regulated diet, and the difficulty of accomplishing this, either with children or persons advanced in life, is well known: indeed, with respect to these last, low diet is not always free from danger. In some persons it produces a nauseous odour, perceptible to the smell when the curtains are opened: it also causes loss of appetite, the tongue becoming at the same time large, pale, and loaded.

The presence of diarrhœa obliges the patient to get up frequently, and thence arise displacements of the cataract. Constipation may have many of the disadvantages which attend cough, and may occasion sympathetic effects besides. The presence of bleeding hemorrhoids contra-indicates the operation; and although it may be practised when the flux ceases, still we must, under such circumstances, always be on our guard against congestion about the head, and combat the slightest symptoms of this by the application of leeches to the anus. When the patient has any herpetic eruption, the operation may determine the eye as the seat of irritation, thus giving rise to serious disease of the organ.

After having combatted the diseases with which cataract may be complicated, (all of which M. Dupuytren states that he is far from having enumerated,) there remains for us to choose between the two methods of operating; for nothing can be less rational than to adopt either universally, and without reference to the circumstances of the individual case. In surgery, as in medicine, the same methods of treatment cannot always be adopted in order to accomplish the same end: thus in cataract, the age of the subject, the form and size of the eye and its appendages, and various other circumstances, may compel the surgeon to have recourse to one form of operation in preference to the other. With regard to age, if we consider the state of the absorbent function, it will be apparent that we should prefer depression in children,

and extraction in elderly people. In the former, the vital functions are in all their energy —composition and decomposition are performed with astonishing rapidity—the absorption of the chystallin commences almost the moment that it is detached; besides which, it is never so hard at this period of life as in old age, and thus is less disposed to resist the powers of absorption. In old people again, the acts of composition and decomposition are sluggish; absorption, in particular, appears to have lost its energy, and the chystallin is of remarkable hardness, and, of course, more slowly acted upon by the absorbents. M. Dupuytren states that he has known the lens perfectly untouched, although displaced for more than two years in elderly persons, who had died of complaints unconnected with the cataract.

There are yet other considerations besides those above mentioned which are in favour of depression in children. They are rarely so docile as to refrain from all movement or struggling during the operation, a circumstance which renders extraction difficult, and which may cause the escape of the vitreous humour. In old persons the eye is deeply imbedded in the orbit, in consequence of the absorption of the adipose substance from the bottom of the cavity: under these circumstances extraction is extremely difficult. Besides, we meet with individuals of all ages in whom, from some preternatural movement or conformation of the ball of the eye, this last method is rendered inexpedient; and without speaking of those who have the eye constantly in a state of agitation, from rapid and convulsive movement, it is a general observation, that as often as an individual is deprived of sight for some time, he seems, with the habit of seeing, also to have lost the faculty of fixing the eye, the motions of the globe not being under the control of volition—a circumstance which much increases the difficulty of extraction.

After these general and comparative remarks on the choice of the two methods, M. Dupuytren described the manner of operating in both. According to him, two instruments suffice; for extraction the knife of Richter—for depression the needle of Scarpa, modified. Richter's knife appears to him preferable to that of Lafaye, because it acts principally by *sawing*, while the other acts rather by *pressure*. The methods themselves are too well known to require description, and we shall only draw the attention of our readers to one point in the operation of depression which M. Dupuytren has illustrated. Scarpa was originally of opinion that all cataracts ought to be broken down. It will easily be seen how much the illustrious Italian was in error, if we consider that, in order to offer a sufficient resistance to the needle, the chystallin would require to be of much more considerable size. On the other hand, the cataract is fixed by bands of the utmost fragility. The parts against which the needle is carried offer much less resistance than the vitreous humour; and if, along with the softness of this last, we take into consideration the difficulty with which some cataracts are broken between the fingers even after their extraction, we cannot but be surprised at the opinion of Scarpa. There are, however, some cataracts which ought to be broken, and which, indeed, it is impossible to depress. Such are those, the cohesion of which presents no resistance to the instrument. After the operation, we ought to be on our guard against determination of blood to the head. In young subjects, the most active antiphlogistics ought to be employed; but in older persons, and where the temperament is not sanguine, these measures ought to be used with moderation. A simple white bandage, with a green or black one over it, suffices to cover the eyes. It is useless, and even hurtful, to apply charpie, which tends to increase the danger of ophthalmia; and from the pressure necessary to keep it in its place, may even occasion the evacuation of the vitreous humour where extraction has been practised.

From the Journal General de Medecine, &c.

SUR L'EMPLOI DE L'IODE DANS LE TRAITMENT DE LA GOUTTE. Par A. N. Gendrin, *réd.*

The virtues of a newly discovered medicine in the treatment of a serious disease, can be accurately determined only by multiplied observations, repeated under varied circumstances, by different physicians. It was in the conviction of this truth, and in order that I might not expose myself to the imputation of having deduced an unwarranted conclusion from a too limited number of facts, that I invited the attention of physicians to the advantages which I had obtained from the employment of iodine in the treatment of gout. These advantages have been confirmed in my own practice. I have varied in divers ways the administration of the remedy, both externally and internally; it has been used in frictions, baths, vapour, tincture, alkaline solution, and enemata, and I have never observed any unpleasant effect produced by it. In all cases where the patients were not cured in a few days, their condition has been rapidly changed for the better. If these cases, which now amount to twenty-six, afford sufficient encouragement for further trials, they are not sufficiently numerous to justify us in attributing definitely to iodine the power of curing gout in all cases. It is for this reason that I have not yet given a detailed account of them, because I am desirous rather to have them corroborated by the observations of others, than to present them as alone sufficient to resolve the question. I shall first, therefore, publish in this Journal, the facts, favourable or otherwise, which may be communicated to me by my *confréres*, and add to them such reflections as they may appear to me to require. In this manner I hope to prevent any undue prepossession that I might otherwise conceive for a medecine, which up

to the present moment I have so successfully employed. I renew, therefore, the invitation which I addressed to the members of the profession generally, requesting them to verify the results which I have obtained, and to transmit to me their observations, that I may lay them without delay before the public through the medium of the *Journal Général.* I begin to acquit myself of my promise, by publishing the two following letters.

Letter from Dr. Louis Valentin to the Editor of the Journal Général, &c.

Nancy, June 15, 1828.

My Dear Sir,—It is with much pleasure that I reply to your invitation, published in the *Journal Général* for April last, relative to the efficacy of iodine in arthritic affections. The appeal which you make to the profession, will doubtless give rise to many communications. In the meantime I send you the following, which dates from a remote period.

In the interval between the years 1784 and 1790, a great number of persons affected with goitre came under my care in the city of Nancy and its vicinity; the soldiers belonging to the infantry composing the garrison, who mounted guard during the night, were often affected with this tumour. Having made many experiments on this occasion, particularly in the composition of the remedies which I employed, as well upon the soldiers as upon the citizens and inhabitants of the country, I observed that those who had nodes, contractions of the fingers, and enlargements of the joints, the sequelæ of gout, were cured, or at least more or less relieved of these affections. Of all the remedies which I employed, sponge, calcined or only burnt, formed a component part. I gave it also to arthritic patients unaffected with goitre, and with good effect. But another kind of *anti-strumous* powder which I prepared with the purified soda of the sponge, acted still more promptly in cases where both diseases were combined, restoring the mobility and suppleness of the articulations; I made use of this composition, and often with success, in patients who were affected with gout alone.

Passing through Geneva in returning from my first journey to Italy in 1820, Dr. Coindet informed me of his plan of treating goitre, by means of iodine; and gave me some of this substance and of its tincture. The odour of the iodine when rubbed, appeared to me to resemble that of sponge, a circumstance which I remarked at the time, and told the doctor that on my return to Nancy I would have the sponge analysed, strongly presuming that it would be found to contain iodine, which would afford an explanation of its success in the treatment of goitre. The analysis was made by two eminent chemists, and iodine was discovered, but a greater quantity was obtained by one than by the other.

I have never employed burnt sponge or iodine externally, in uncombined arthritic affections, but since my first observations I have often prescribed during the chronic stage of gout, or with a view to prevent its return, the carbonate of soda in conjunction with a bitter powder, or with an opiate; most commonly I have directed it to be dissolved in an infusion of the roots of *calamus aromaticus*, or of *gentiana lutea*. For a long time I have not given the sponge to arthritic patients, unless they were at the same time affected with goitre.

If any one should be inclined to doubt the date of my little discovery, he may consult my manuscript upon goitre, sent to the Royal Academy of Surgery in 1789, for which a gold medal was decreed to me by that body. If sponge contain iodine, then I discovered forty-three years ago, that this substance, which was not known till 1813, is useful in arthritic affections. I am, &c.

The salutary effects resulting from the employment of calcined sponge in the treatment of gout, with which the patients of Dr. Valentin were accidentally affected, evidently confirm, as this able practitioner supposes, the results which I have obtained from the use of iodine in gout, for it is certain that calcined sponge does contain iodine, but it also contains a considerable proportion of alkali, and this latter substance is even in greater quantity than the iodine, so that it also, may lay claim to a participation in the success. It has indeed long been known that alkaline beverages have affected a cure in some cases of chronic gout, especially when there existed swellings of the joints; this consideration detracts much from the importance of the facts communicated by M. Valentin, which moreover were evidently not of a nature to lead to the employment of iodine, which I was solely induced to use from the fact of its generally recognised utility in chronic engorgements of the glands and articulations.

Letter from Dr. Godier to the Editor of the Journal Général, &c.

Paris, June 22, 1828.

Sir,—I have the honour to communicate to you the commencement of a case confirmatory of the anti-arthritic virtue of iodine; the success has been too prompt to permit me longer to withhold the details of the case, although the cure is still incomplete.

Delaunoy, a woman about 60 years of age, of a sanguineous temperament, and living in an unhealthy street, was obliged, from her occupation, to have her hands frequently immersed in water; she had a paroxysm of gout, which, although the first which she had had, attacked successively all the articulations. The toes of both feet were red and tumefied; the aponeurosis plantaris at its attachment to the os calcis was swelled, but without redness, and was extremely painful, as were all the affected parts; the joints of the fingers were likewise red, tumefied, and painful. I directed absolute repose, a sudorific ptisan, and the application of flannel to the suffering parts; no change resulting after the lapse of several days, I had decided to have recourse to leeches,

when I read in the *Journal Général, &c.* your observations on the anti-arthritic properties of iodine. I determined to make trial of the remedy, and the same evening prescribed frictions upon the affected parts, with an ointment composed of one part of iodine and twelve parts of axunge. Four days afterwards, the tumefaction began to diminish, together with the pain; the frictions were employed night and morning, and to-day, 22d June, she informs me that she is almost entirely free from pain, and that she can use her needle, which before the employment of the ointment she could not do; she is still not entirely cured, but such a surprising effect, obtained in the space of fifteen days, by the external use alone of iodine, has induced me to request a place in your Journal for the commencement of a case, the conclusion of which shall be forwarded to you as soon as the cure is complete.

I am, &c.

GODIER, D.M.P.
Médecin du Bureau de Charité du Premier Arrondissement.

P. S.—June 29. Since my last communication, the patient has been alternately better and worse; she is worse in stormy weather particularly. For the last three days I have given iodine internally, and she is improving daily. I shall avail myself of another opportunity to communicate to you the sequel of this case, together with several others, all favourable to the use of the remedy.

The success obtained by M. Godier is the more remarkable, since the quantity of iodine applied externally was so small; it is not to be wondered at that the symptoms should re-appear, when the immediate effect of the medicine has been somewhat diminished by habit; and to complete the cure but two methods remained, either to administer the remedy internally, as was very properly done by M. Godier, or to increase the proportion of iodine in the ointment. In the latter case there is one remark which my experience leads me to make.

When the proportion of iodine in the ointment exceeds one-eighth part, it is too irritating for the skin of most patients, especially when the frictions are always made upon the same place; in such cases it will be advisable to apply them alternately to the inner part of each thigh and arm, alternately, or the ointment may be made less irritating; this may be accomplished by using as an excipient for the iodine, *baume tranquille*, mixed with an equal part of axunge. It is under this form that I now apply the remedy to joints affected with acute gout.

From the London Medical and Surgical Journal.

FUNGUS HEMATODES—LIGATURE OF THE CAROTID ARTERY. By M. LISFRANC.

Josephine Lenoir, æt. 18, of a lymphatico-sanguine temperament, enjoyed perfect health until she arrived at the age of twelve. At this period there appeared in the situation of the right parotid gland a tumour, accompanied with slight cephalalgia, throbbing in the right lateral region of the head, and palpitations of the heart. This tumour was supposed, by the practitioners who were consulted, to be engorgement of the lymphatic vessels of the neck. From the first appearance of the malady, until 1827, hemorrhage took place at various times from the meatus auditorius of the side affected, which appeared to ease the pain from time to time, without arresting the development of the tumour, the pulsation in which becoming more manifest, inspired the friends of the patient with serious inquietude. Resorting to Paris to obtain the succours of the surgical art, she entered the hospital of la Pitié on the 11th of January, 1827, in the following state:—From the angle of the lower jaw to the lobule of the ear, the tumour is soft, even, round, without change of colour at the base, of the size of a hen's egg, diminishing on pressure, exhibiting pulsations synchronous with those of the heart, which pulsations, without quite ceasing, are less perceptible during the compression of the primitive carotid artery; pulsations otherwise stronger, more frequent, and extensive, than in the natural state. When the tumour was compressed, no effect appeared to be produced on the brain. All these circumstances gave rise to an opinion that this was an aneurism of the external carotid artery. (*V. S.* ℥viij., *and low diet* from 12th January to 10th April.) Ligature on the common carotid, which was intended to be had recourse to, was deferred, owing to the lowness of the temperature of the atmosphere and other circumstances, which left time to observe the disease with more attention. Several distinguished practitioners were consulted on the question, whether or not the heart was healthy? It was their unanimous opinion that this organ was in the natural state. The beating of the common carotid, which appeared to extend to about two inches and a half in all directions below the tumour, rendered it rather suspicious that this vessel was also affected: in fine, an abundant discharge of blood from the meatus auditorius of the same side gave rise to some inquietude. On examining with attention the interior of the ear, a small, soft, reddish tubercle was discovered, formed of an erectile tissue. This gave rise to a suspicion that the tumour, regarded before as an aneurism, was a fungus hematodes. As it appeared evident, from the circumstances attending the case, that, if abandoned to the efforts of nature, it would lead to certain death, M. Lisfranc thought that some chance would be afforded to the patient by applying a ligature on the carotid artery. In a case so serious and complicated he, however, not wishing to rely entirely upon his own views, presented the patient to the Académie Royale de Médecine (Section of Surgery,) the members of which were of opinion that the operation ought to be had recourse to. The greater

number of the distinguished surgeons who saw the case thought that the tumour was an aneurism, and not a fungus hematodes.

April 10th, in the presence of several members of the academy, the operation was performed in the following manner:—The patient, lying on her left side, contracted the sterno-mastoid muscle, in order to render its anterior edge more distinct, along which M. Lisfranc made an incision of three inches long, in the middle of the neck. The skin, the sub-cutaneous cellular tissue, and the fascia cervicalis were incised with precaution; a very considerable venous plexus, observed under this tissue, was moved aside with great care, and held by the finger of an assistant towards the upper angle of the wound. A sponge squeezed out of cold water was now applied for a few minutes to the wound, by which means the bleeding of the small vessels was stopped, and the surface of the wound looked as clean as if it had been on the dead body. The sheath of the artery was divided with great care according to the method of Scarpa. The cellular tissue surrounding the vessel was moved aside with the fingers, and the sterno-hyoidien muscle was pressed inward and downward out of the way. The jugular vein, known by its colour, its softness, and the augmentation or diminution of its size, as the patient exerted herself or not, was more inward. A large vein was discovered passing across the upper part of the wound; the cellular tissue surrounding it was removed with great precaution, and two ligatures were applied; the vein was then divided in the interval. The internal jugular vein, moved outward, permitted a ligature to be carried under the artery, from without inward, with great facility. The operator, having satisfied himself that the artery, and nothing but the artery, was included in this ligature, tied the vessel very tightly. The pulsations, contrary to the opinion of some authors, did not completely cease in the tumour, the size of which had already diminished one half. The pneumo-gastric and the great sympathetic nerves were observable at the bottom of the wound. The extremities of the ligature were allowed to hang out at the lower angle of the wound. The edges of the wound were now brought together by two strips of adhesive plaster, and a compress and bandage applied.

The pulse, examined by M. Moreau during the whole of the operation, did not undergo any change. The patient, in every respect, showed uncommon courage. From ten o'clock until one in the afternoon, she felt remarkably well; the throbbing in the head less painful; countenance smiling; pulse as natural as before the operation; slight pain only in the wound. (*Gum water*; *absolute abstinence.*) At one o'clock, pulse frequent, quick, full, hard; face flushed; injection of the conjunctiva; eyes full of tears; cephalalgia. (*V. S.* ℥viij. *from the arm; application of ice to the tumour.*) At three o'clock, the patient felt a little pain in the throat. (*The ice to be discontinued.*) At six o'clock, some slight shiverings; perspiration; feeling of syncope; severe pains in the epigastric region and towards the sternum; nausea; cephalalgia; pulse full and accelerated; countenance pale. At eight o'clock, the countenance looked as before the operation; the epigastric and substernal pain less severe. (*The same drink.*) She passed the night very quietly. 11th. Difficulty of deglutition diminished; epigastric pain continues; pulse frequent; face flushed and swelled; injection of the superficial veins; cephalalgia relieved by three bleedings from the nose, which took place between nine o'clock in the morning and noon. At this moment, throbbing in the head; cephalalgia; dyspnœa; and a sense of suffocation. (*V. S.* ℥viij. *from the foot, followed by great relief.*) The wound dressed morning and evening. Towards the evening the symptoms returned. (*Eight ounces of blood taken from the foot.*) Marked relief; two hours' sleep during the night. 12th. Pulsation weaker in the tumour, which appears a little diminished in volume; pain in the throat quite gone. (*Vegetable lemonade for drink.*) In the course of the day, the head became affected again with severe pain; pulse full and frequent; skin hot and moist; palpitations. (*V. S.* ℥viij. *from the foot, followed by a remarkable amendment; dressing the same.*) 13th. Substernal pain less acute; the skin continues hot; pulse frequent and full; cephalalgia persists. (*V. S.* ℥viij. *from the foot.*) Complete cessation of the pains. The aspect of the wound looks favourable; it is dressed twice a-day. 14th. Slight sleep during the night; the pain of the stomach and chest is now only intermittent. (*Emollient drink.*) In the day pulse frequent; face flushed; ardent heat of the skin; veins of the face much injected; sense of general weakness and of constriction in the chest. *In the evening, V. S.* ℥iv. *from the foot.*) This was followed by slight syncope, though with sensible amelioration. 15th. Pulse less frequent and less full; tumultuous throbbing in the precordial region; general illness; anxiety. (*The same drink; antispasmodic mixture.*) 16th. Pain the neck, attributed to the constant position of the patient on the back; considerable debility. The wound looks well, and the suppuration is of a good quality. 17th, at four o'clock in the morning, painful oppression; pain in the right side of the thorax; intense cephalalgia. (*V. S.* ℥vj. *from the foot.*) This was followed by inexpressible relief. At eight o'clock, in the same state. (*Few spoonsfuls of buoillon; diluent drink; antispasmodic mixture.*) On dressing the wound, a point near the ligature looked very angry, which gave rise to a suspicion that a coagulum had not yet formed. In the evening, pain in the throat; prostration of strength; pulse less accelerated; contractions of the heart less violent. 18th. No difficulty of deglutition; the same state of the heart and pulse. At five o'clock in the evening, countenance thin; swelling of the veins of the forehead; slept for an hour. At this time the patient wished to drink; in lifting

her head to do so, she felt slight pricking in the wound, and dreadful hemorrhage immediately ensued. The dressing was immediately removed, and the finger of the *religieuse* pressed in the wound was not able to arrest the flow of blood. The house-surgeon immediately arrived and found the patient in a state of syncope; the hemorrhage had stopped; inspirations deep and rare; pulsations of the heart slow and feeble; pulse small and very compressible. (*Aspersion of the face with vinegar and water; friction on the precordial region and of the limbs.*) The pulse became insensible; the heart ceased to contract; at a quarter before eight all attention was now useless; the patient was no more. The tumour had diminished one-half.

Sectio Cadaveris, forty-five hours after death.—Embonpoint ordinary; slight rigidity of the limbs; skin blanched. *Head:* The base of the cranium presented a very remarkable alteration. The petrous bone of the right side had tripled its natural size; its superior edge was on a level with the small wings of the sphenoid. Its tissue was soft, friable. On detaching the dura mater, fragments of this bone came away adhering to the membrane. The interior of this bone was spongy, and of a reddish colour, resembling the cavernous portion, slightly .macerated. The cavernous sinus of the same side was flaccid at the internal extremity of the petrous portion. This portion extended to the posterior clinoid process, both of which appeared to form only one piece of bone. The disorganization of the bone in this part was such that a probe could be run in many points from the interior of the cranium to the cervical region through preternatural apertures. The thyroid gland was apparently healthy. The common motor oculi, the pathetic, the trifacial, and the external motor oculi nerves, did not present any sensible change of structure; but the acoustic and facial nerves were at least twice their natural size at the point before entering the petrous bone. The dura mater was much thickened in the right temporal fossa, and so adherent to the petrous portion as to render it impossible to separate the two without tearing off fragments of the bone. All the sinuses were enormously dilated, particularly those of the diseased side. The superior and inferior petrous and the cavernous sinuses of that side were filled with an erectile, spongy substance, divided into an infinite number of small cells, all of which communicating with each other; by insufflation, they could be distended into a polygonal form: they contained hardly any blood. The arachnoid and pia mater were thickened and adherent to each other over the lower surface of the middle lobe of the brain on the diseased side. *The brain did not entirely fill the cranium.* An interval of a quarter of an inch separated the cranial, from the cerebral, arachnoid. The brain was otherwise in the natural state, its colour only appearing rather paler. *Neck:* The fungous tumour was situated in the hollow between the posterior edge of the lower jaw, and the mastoid process, extending to the lower surface of the petrous portion of the temporal bone and the auditory conduit, to which it adhered by strong prolongations. Between the ramus of the jaw and the tumour was observed the superficial temporal artery and vein; the facial nerve enveloped in a hard cellular tissue; several veins in a remarkably enlarged state; the stylo-maxillary ligament, and the cervico-facial branch of the respiratory nerve. The internal carotid artery traversed the substance of the fungus, in which it gave of three thick branches; to the jugular vein, which contained within its canal a considerable prolongation of the tumour; to a great number of accidentally developed veins; to the pneumogastric and to the superior and middle cervical ganglions. The parotid gland was found in a state of great atrophy, reduced to a very small size. The skin over the fungus was healthy. The tumour, since the hemorrhage, had diminished very considerably. Divested of its cellular covering, and of the surrounding vessels, its size was not larger than that of a hen's egg. Its form and appearance might have been compared to those of the heart of a three-months fœtus. Its structure was soft, spongy, of little consistence, diminishing under the pressure of the fingers, of a dirty red or brownish colour. It was formed partly of aneurismatic vessels and partly of erectile tissue; it had no proper capsule. Its cells communicated with each other, and appeared to consist entirely of arteries and veins in a dilated state, which formed the chief constituents of the fungus. The upper two-thirds of the wound resulting from the operation was cicatrized; the rest was filled by a fleshy substance. There was no abscess or purulent secretion any where surrounding the part. The ligature still embraced the artery; and, what was remarkable, the rupture had taken place fifteen lines below the ligature, and a coagulum filled the caliber, otherwise healthy, of the vessel, from the ligature to the opening where the hemorrhage had issued. *Thorax:* In the posterior mediastinum, some of the lymphatic glands were found softened, containing caseous matter, which had no connexion with the wound of the neck. All the other parts were healthy. *Abdomen:* All the abdominal viscera were in the natural state; a great quantity of gas had formed in the digestive canal and in the peritoneal cavity.

We have translated this very interesting case nearly at full length. It is very probable that, had hemorrhage not taken place, the patient must have soon died of the disease of the brain and of the bone. The case is altogether one of great interest.

From the Gazette de Sante.

OBSERVATION SUR UNE HYDROCEPHALE GUERIE PAR LA FORMATION SPONTANEE D'UN ABCES. Par M. Auguste Larrey, D. M. á Toulouse.

June 20, 1827, I was requested to visit

the child of M. Pialhés, æt. 14 months, who had been much indisposed for the preceding five or six days, up to which period she had enjoyed excellent health. Previously to sending for me, a variety of anthelmintics had been given, on the supposition that she was troubled with worms, but without bringing any away. The following were her symptoms when I saw her; pulse strong and quick, profound coma, occasional screams, frequent convulsions, respiration natural, abdomen soft, and not at all tender upon pressure. The parents assuring me that the child had not been exposed to the heat of the sun, nor had in any way injured her head, which, notwithstanding, appeared to be the seat of the disease, I was, at first, unable to determine its character; acting, however, upon existing symptoms, I directed the application of six leeches behind each ear, an exclusive milk diet, and a few drops of orange flower water, to mitigate the extreme thirst with which she was harassed.

The above symptoms continued unabated on the 21st, and others now made their appearance; every thing swallowed was immediately ejected from the stomach, the alvine and urinary evacuations were suppressed, constant stupor, convulsions more violent than before, pupils dilated, and the head drawn backwards. A blister was directed to the back of the neck, ice to the head, and sinapisms to the legs; in the evening a blister was applied to the right arm, as a substitute for that on the neck, which it had been impossible to retain in its place.

No change in the melancholy condition of the patient, was perceptible on the 22d; her pulse was softer, and the abdomen appeared tense; emollient fomentations were ordered, the blister kept open, and the other remedies continued.

On the 23d, there was an aggravation of all the symptoms; the convulsions were more frequent and of longer duration, the vomiting continued, and the child, when allowed a moment's repose, appeared as if lifeless. The nurse now confessed that about the beginning of the month, the child had struck its head by a fall from her arms upon the ground. A blister was applied upon the head, and frictions with camphorated oil upon the abdomen; the patient readily took the breast, but immediately rejected what it had swallowed.

Nothing occurred worthy of notice on the following days; the blister on the head soon dried up, and one on the left arm was substituted. On the 28th, the mercurial treatment was commenced; calomel was given internally morning and evening, and frictions with *Ponguent napolitain* were made regularly once a day, both along the spine and behind the mastoid processes, at first in quantity of a quarter of a drachm, afterwards of a drachm. By the 9th of August, twelve days afterwards, she had consumed five drachms of the ointment and 18 grains of calomel, without any marked effect being produced, except that the convulsions and vomiting were not so frequent as before. The above treatment was suspended during four days; at this time the unfortunate patient had become so greatly emaciated, that I did not think it necessary to trouble her farther with medicine. The mercurial treatment was recommenced on the 17th, and in the space of seven days, six drachms of the ointment were consumed.

Perceiving the inutility of all our efforts, and that the only remaining resource was the hope that, from the protracted character of the disease, nature was about to establish a crisis, every thing was discontinued with the exception of emollient fomentations to the abdomen, and death was daily expected to terminate a life of so much suffering.

Finally, on the 31st July, 41 days from the invasion of the disease, and about two months after the fall, an oblong tumour, about the size of a pigeon egg, was observed on the inner and upper part of the left arm; it was painful to the touch, and attended with inflammation of the skin; the day following it had increased to twice its former size, and when opened, on the 2d of August, about eight ounces of a white and very thick pus were discharged. The wound was dressed in the usual manner, and by the 7th, was completely healed. From the day on which the abscess was opened, the patient appeared as if restored to life, she called urgently for food, and greedily devoured all that was set before her. This appetite continued without any unpleasant consequence for the space of ten or twelve days, when convalescence was finally established, and after the lapse of a month she was entirely cured.

From the Glasgow Medical Journal.

CASE OF LITHOTOMY, COMPLICATED WITH OBSTRUCTED BOWELS, *from the pressure of an Osseous Tumour in the Mesentery*. By John Macfarlane, M.D. Lately one of the Surgeons of the Glasgow Royal Infirmary, &c. &c.

Robert Cameron, æt. 67, weaver, was admitted into the Royal Infirmary, on the 23d of January, 1827, on account of stone in the bladder. Complained of severe pain about the neck of the bladder, and at the glans penis, coming on frequently when at rest, and without evident cause, but always urgent during micturition, when at stool, or on the slightest motion of the body. The calls to void urine varied in frequency. For whole days he required to pass it every quarter of an hour, generally in a small stream, frequently obstructed, and accompanied by painful tenesmus. Had slight pain on pressure in the situation of the right kidney, and had several times voided small calcareous particles about the size of a pin head, and twelve months before, a hard smooth yellow stone, of the size and shape of a kidney bean. A large sound was readily introduced into the bladder; and from the irregular feeling and rattling noise

communicated, it was evident that there were several calculi. The urine was of a natural colour, but on standing deposited a small quantity of flaky sediment. The bowels were obstinately costive, and the abdomen somewhat tympanitic. These symptoms, commencing about 19 months before his admission, and gradually increasing for the last six weeks, had forced him to give up his employment.

On attentively examining this patient, to ascertain if any other disease existed likely to militate against the success of an operation, it was found, that with the exception of a slightly enlarged prostate, and flatulent distention of the bowels, no other morbid manifestation could be discovered; and these were not such as to forbid an operation, to which he was anxious to submit. Although advanced in years, he still exhibited a healthy and robust appearance, and possessed sufficient vigour to sustain the shock of an operation, and afterwards to establish an efficient process of renovation. The straining on going to stool, and while micturating, was excessively severe, and resembled much the propulsive pains of parturition; but it was believed that the obstinate constipation, and the irritation from the calculi and enlarged prostate, were sufficient to account for this unusually urgent symptom. The prostate gland had not the globular shape usually observed; it was, however, somewhat enlarged, firmer than natural, and so flattened, that the finger could not reach the bladder. When this part is in a state of simple chronic enlargement, we cannot reasonably refuse our patients the chance of an operation, should there exist no other unfavourable combination. I have twice operated with success in more extensive enlargements of the prostate; and although the wound did not heal so speedily as when no such disease existed, the result was still sufficiently fortunate to justify the operation, and in one of the cases, the gland diminished considerably afterwards. The existence of several calculi in the bladder would, by requiring the frequent introduction of the forceps for their removal, somewhat protract the operation, but this could afford no ground of apprehension, it being acknowledged that there is more danger to be dreaded from the extraction of one large, than of many small calculi.

The patient was subjected to the usual treatment for a few days. He was repeatedly purged with castor oil and enemata, and an immense quantity of scybala evacuated, but without any marked reduction of the flatulent distention of the belly; the bladder was soothed by the warm bath, anodyne clysters, and frequent doses of supercarbonas sodæ.

On the 29th, the lateral operation was performed, by running the narrow probe-pointed knife along the groove of the curved staff, and six entire, and three broken calculi extracted, the largest being oval, and about the size of a walnut. From the enlargement of the prostate, and depth of the peritonæum, some difficulty was experienced in feeling with the finger the whole internal surface of the bladder, to ascertain that all the calculi were extracted. To remove all doubts in such cases, the introduction of a sound, either by the penis or the wound, should not be neglected, as it affords the most correct means of ascertaining that this necessary object has been effected. The prostate gland felt hard, almost like cartilage, but when the finger or forceps were withdrawn, it still retained so much elasticity, as to close the wound into the bladder as if by a valve. Three arteries were observed to bleed freely, two superficial branches were tied, and a large deeper-seated vessel, evidently the transversalis perinæi, threw out its blood per saltum, and in considerable quantity. This artery was much enlarged, but from its deep situation, and its being divided near to the ramus of the ischium, it was found impossible to secure it by ligature; it was, however, easily commanded by pressure. An elastic tube was introduced into the bladder; the patient was placed on his back in bed, the thighs separated, to facilitate the escape of blood, and he was lightly covered, and kept cool. On visiting him at 8 P.M. the urine was passing freely along the tube, and he was free of pain, except when occasionally attacked by a strong bearing-down sensation, accompanied with a desire to void urine and go to stool, which being similar to what he experienced before the operation, was ascribed to flatus. The finger was passed into the bladder, which was found empty. Pulse 72, soft. Complained of thirst, and slight rigours. *Anodyne enema.*

30th. Has had some sleep during the night, and feels easy, except when affected at intervals with severe spasmodic pains in the abdomen. In the evening, as he had pain on pressure above the pubes, and his pulse was accelerated, 24 leeches were applied to the hypogastrium, and a *large enema* ordered, which dislodged a quantity of hardened fæces.

31st. Continues to complain of violent expulsive efforts, and of fixed pain above the pubes. Pulse 74—tongue clean—no stool. The tube was withdrawn. *Castor oil. Leeches to the hypogastrium. Anodyne enema at bedtime.*

1*st February.* Six stools from the oil, but continues to complain of fixed pain in the hypogastrium, and of general uneasiness in the abdomen from flatulence. *Leeches and anodyne repeated.* A large elastic tube was introduced for several inches into the rectum, to facilitate the escape of air from the bowels, but only a small quantity was discharged. This practice is sometimes successfully adopted, when the natural peristaltic action of the intestines has been impaired by flatulent distention; but if the gas is confined in the small, or high up in the large intestines, beyond the reach of the tube, no benefit can be expected from its introduction.

2d. Passed a comfortable night; but an hour before the visit had a small rigour, followed by increase of pain, thirst, and nausea. Pulse 84, small and sharp. Tongue dry and

furred; *Calomel and opium. Fomentations. V. S. to ℥vij.* Blood cupped and buffy.

3d. Pain on pressure, swelling, and tenesmus greatly abated. Voids his urine through the wound at intervals, by contraction of the bladder. Pulse 72, soft and compressible.

5th. Flatulence and tenesmus increased, coming on in frequent and violent paroxysms, but with little or no pain on pressure. Has had several stools, containing scybala, accompanied with excruciating pain during their evacuation. Complains of the feeling of a large hard body, fixed in the upper part of the rectum, which excites violent expulsive efforts. Pulse 70, rather weak. Tongue dry and furred. Wound sloughy. *Dose of castor oil, and anodyne enema after its operation.* These symptoms appeared to depend on abdominal irritation, the consequence of an impacted state of the colon; but did this condition of the bowels afford an adequate explanation of the violent bearing-down efforts, which had annoyed him more or less for a whole year?

From this report till the 10th, there was little change in the symptoms. The spasmodic pains affected him violently, and the wound was lined with an ash-gray tenacious secretion. The stools still contained scybala; his countenance was pale, and had an exhausted expression, on which account purgatives were more sparingly administered, and the bowels unloaded by frequent and copious injections, thrown freely up by the patent enema syringe.

13th. Paroxysms of pain continue unabated, but the stools are now of a natural colour and consistence. Pulse 68. *Anodyne enema. Beef tea. Arrow root.*

15th. Had a violent attack of pain this morning, chiefly referred to the rectum, and he describes it as exactly similar to what he experienced on going to stool previous to the operation. The finger was passed into the rectum, but neither hardened fæces, nor any other obstruction, was discovered. As the bowels were now acting more freely, and the flatulence had diminished, and as he complained of burning heat about the prostate and bladder, it was judged proper to sooth the recto-vesical irritation by a pill every six hours, containing *Extract. hyosciami gr. iij. and camphor gr. ij. Warm bath. Anodyne enemata. An occasional opium suppository.*

For the following three days, the attacks of pain were less violent, and he appeared to improve in strength and spirits; he still, however, complained of severe straining at stool, from the feeling of a foreign body in the gut, which he was ineffectually excited to expel. On the 20th, his appearance was less languid; his pulse was about 80, and of moderate strength, the tongue clean and moist, and the wound florid and granulating. The thighs were secured together, to accelerate its closure, and the urine passed afterwards by the penis. His appetite was improving; and although at this period he was considerably exhausted, there existed no prominent indication of a suddenly fatal result. On the morning of the 22d, he was seen by the nurse at six o'clock in his usual state, and when visited again at eight, he was found dead in bed.

Dissection.—On opening the abdomen, a hard tumour was discovered lying over the last lumbar vertebræ, between the laminæ of the mesentery, near the inferior part of the ilium, and which pressed on the sigmoid flexure of the colon, where it is about to become rectum. The surrounding mesentery exhibited no thickened or diseased appearance, and only adhered to the surface of the tumour by loose cellular attachments, easily destroyed by the finger. It was about the size of a small lemon, of a hard bony feeling and appearance, and very irregular shape. When sawn through, the exterior part was evidently bone, and varied in thickness at different parts from a quarter to half an inch, while the centre was filled by a yellowish white substance, in appearance and consistence like adipocire, intersected in various directions by spiculæ of bone. Two small cavities in the centre were lined with innumerable transparent, needle-like crystals, which, however, disappeared after the tumour was dried, and before I had an opportunity of submitting them to chemical analysis. The mucous coat of the bladder was considerably thickened, of a dark vascular plaited appearance, especially about the neck, and coated by a muco-purulent secretion. There was a tumour at the fundus about the size of a small marble, containing purulent matter, which issued into the cavity of the bladder, through two fistulous openings in the mucous coat at that part. The prostate gland was enlarged, and firmer in texture than natural, but without the fibrous appearance of scirrhus. The mucous coat of the rectum was highly inflamed, and there was considerable induration and thickening of parts between this gut and the base of the bladder.

This dissection afforded a satisfactory explanation of what had been previously only matter of speculation. The long-continued and painful tenesmus was obviously to be referred to the pressure of the osseous tumour, on the commencement of the rectum, producing an impediment to the regular discharge of the fæces, tympanitic swelling of the abdomen, and great irritation. From the situation and connexions of this tumour, it would appear, that when the diaphragm and abdominal muscles were called into action in expelling the fæces, it would be forced back on the termination of the colon, by the pressure of the surrounding parts, and not only impede the fæculent evacuations, but also, from its extreme hardness and inequality, irritate and injure the bowel in no small degree.

In scrofulous habits, the mesenteric glands are sometimes filled with calcareous matter, but bony depositions are stated by Dr. Baillie (Morbid Anatomy, p. 134,) to be of rare occurrence. The few recorded cases of this disorganization, which I have had an opportunity of examining, appear to have originated in disease of the glands of the mesentery, and

to have been complicated with organic disease of the bowels. Dr. Donald Monro narrates a case in the Medical Transactions (vol. ii. p. 361,) in which all the mesenteric glands, varying in size from a pea to a walnut, were hardened and ossified. They were not, howver, as in the case of Cameron, made up of one large firm osseous tumour, but, "like spongy carious bones, they were composed of a number of small pieces, joined together by membranes."

Cameron's death cannot be attributed to the operation; he lived for 23 days after its performance; and although the bladder was partially diseased, yet he was exhausted and carried off by an unusual and unexpected occurrence. It was a combination that could not have been detected during life, otherwise no operation would have been performed; and although it had been discovered, it was irremediable. The flatulent distention of the belly of course prevented its being recognised by an external examination, and it was too high up to be reached by the finger in the rectum.

I am indebted to Professor Thomson for the following analysis of the calculi and tumour.

The calculi consist chiefly of uric acid. But there is present in them also a small quantity of matter, which has a light yellow colour, and dissolves with ease both in nitric acid and in caustic potash. But it did not crystallize with either, nor form the pink coloured matter with nitric acid. It may be new; but the quantity upon which I experimented did not admit any farther trials. The bone is very solid externally, and is surrounded by a periosteum in the usual manner. It becomes more and more porous towards the centre. The specific gravity of the whole mass is 1.219. But it was so full of cavities, that this specific gravity is doubtless below the truth. The matter in the middle of the bone is soft, but compact. It cuts like cheese, and is partly buff coloured, partly white. It was not in the least soluble in boiling alcohol, and therefore was not adipocire. It was insoluble in acetic and muriatic acids, and therefore was neither muscular nor ligament. But when digested in caustic potash, a little fat was separated. It melted when heated, and behaved like cartilage.

From the London Medical Gazette.

CASE OF ENLARGED BLADDER. By J. B. Estlin, F.L.S.

To the Editor of the London Medical Gazette.

Sir,—As the following case of enlarged bladder may prove interesting to some of my professional brethren, I have much pleasure in giving it publicity through the medium of the Medical Gazette.

A gentleman, 54 years of age, consulted me in October 1827, in consequence of constant nausea and loss of appetite and strength. His tongue was foul and his bowels confined. The pulse indicated no morbid symptom. I ordered him some cathartics with calomel, and when he visited me two days afterwards he was somewhat better. I then prescribed for him an emetic and a bitter aperient infusion.

October 8.—Not much better. He informed me that for many months he has had some difficulty in passing his water; that a considerable quantity comes away in the day and night, but in small portions at a time, and often involuntarily and without any force. He assured me (and I place full reliance on the declaration) that he had never laboured under gonorrhœa or any other form of venereal complaint.—Repeat the cathartics.

15th.—No better. Being anxious to ascertain the state of the urethra, I introduced a middle-sized bougie, which met with a degree of obstruction at six inches from the orifice that moderate pressure could not overcome; and as much pain was occasioned by the attempt, I desisted from it for the present.

18th.—I introduced a silver catheter, and found it pass into the bladder without any obstruction. A pint of urine was drawn off—a quantity much exceeding what he has passed at one time for many months.

19th.—He suffered much pain after the introduction of the catheter, and experienced not the least relief from the quantity of water removed from the bladder.

It was my intention to have passed the catheter again to-day, principally with the view of ascertaining if there were any calculus in the bladder impeding the passage of the urine into the urethra, but the canal remained in a very uneasy state from the employment of the instrument yesterday; and as he was under the necessity of going a journey on business in a day or two, I thought it better to delay the attempt.

30th.—He returned from his journey last night, in all respects worse. He has constant nausea, and he frequently passes urine involuntarily.—Cap. pulv. ipec. comp. gr. xii. h. s.

31st.—Slept. Vomiting came on this morning and continued through the day. Bowels confined. Calomel, with other aperients, was prescribed.

Nov. 1st.—Vomiting very frequent. Bowels do not act. Calomel and opium given.

2d.—Vomiting incessant: the quantity brought up from the stomach is far more abundant than the fluid he swallows: the rejected matter is of dark colour and coffee-ground appearance. He has some slight alvine evacuations of similar fluid. A few ounces of blood were drawn from the arm: it was buffy. No relief experienced from the bleeding.

3d.—He becomes worse: the vomiting is unabated, and the ejecta are darker. The urine flows involuntarily, from two to three pints apparently in the twenty-four hours.

From the commencement of the vomiting he has had no power of taking food. Various liquids have been tried: soda water remains longest on the stomach.

Yesterday or to-day he directed my atten-

tion to a swelling in the abdomen, which had escaped my notice when I felt the epigastric region, and when I daily pressed the bowels to ascertain if any tenderness existed. I examined the tumour, and found it to be of an oblong form, situated in the right hypochondrium, about the outer edge of the rectus muscle, extending nearly from the eleventh rib to the right side of the symphysis pubis, and being particularly prominent about the situation of the inner abdominal ring. It somewhat distended the integuments so as to be perceptible to the eye, and might be considered to be about three inches in width.

His account of this swelling was imperfect, but he believes that he first discovered it last week, while he was absent on his journey. I was unable to satisfy myself as to its nature. It did not answer to the description of any kind of hernia. It was not elastic, nor could any fluctuation be discovered: it seemed to possess considerable solidity. No inflammation existed, as pressure did not detect any tenderness, nor was there any unusual tension over the rest of the abdomen. Turpentine injections were administered, and cathartics and opium taken by the mouth. The stomach rejects every thing, and the bowels are but slightly evacuated.

4th.—Worse in all respects; pulse 100; countenance bad; was bled again. Injections continued; no fecal evacuations; urine flows plentifully, but generally involuntarily.

5th.—Vomiting incessant. His strength appears to be rapidly giving way. No sustenance can be retained. Tongue brown. Pulse small. The tumour is larger, or the parietes of the abdomen, by sinking in, in consequence of his great emaciation, make it more apparent.

6th.—I was desirous of having another opinion on the case, and he was visited by my friend Mr. J. C. Swayne, surgeon of this city. Upon an attentive examination, as far as we could come to any conclusion, the tumour appeared to be a mass of internal disease, agglutinating the contiguous parts, pressing upon the bladder, and impeding the action of the intestines. By both of us the patient's speedy dissolution was expected. To his friends and himself the same event appeared so certain that he made a final settlement of his affairs with considerable effort. For the last two or three days he has spoken as if he anticipatad a fatal termination. Small but frequent doses of cathartic extract, with opium and purgative injections were ordered.

7th.—He becomes still worse; some delirium; urine continues to be evacuated, and there is no swelling immediately above the pubes. With the view, however, of exactly ascertaining the state of the bladder, and of assisting, by drawing of the water that might be there, the action of the bowels, we resolved upon introducing the catheter. So near did his death at this time appear to his friends, that they earnestly entreated he should be subjected to no further inconvenience, but allowed to have an undisturbed release. These objections were of course overruled, and I introduced the catheter. It passed without any difficulty, and a forcible flow of urine through it occurred. The tumour immediately began to subside, and by the time about three pints of water had been drawn off it entirely disappeared.

The general nature of the disease was now apparent. It could not be doubted that the tumour was a preternatural enlargement of the bladder, and it seemed most probable that the elongated part was the internal coat protruded through the muscular coat; in consequence of which, the natural efforts of the bladder to expel its contents forced them into this cavity, instead of overcoming the cause of resistance at the neck of the bladder. To what extent any morbid impediment existed at the neck of the bladder it was not easy to determine. The catheter passed without obstruction, and examination per anum detected no disease of the prostate gland.

In a few hours, when the tumour began to form afresh, the urine was again drawn off; the vomiting lessened, and the pulse in the course of the day became firmer.

8th.—Vomiting less frequent; urine drawn off night and morning; the vesical tumour is formed some hours before the introduction of the catheter; some feculent evacuations followed the enema.

9th.—Vomiting nearly ceased; feculent discharges after the enemas; no power of voiding the urine, but it flows involuntarily upon the re-appearance of the swelling. He takes nourishment.

13th.—No vomiting; good alvine evacuations from the injections. He was taught to introduce the catheter himself, and directed to empty the bladder every five or six hours, so as to prevent the formation of the tumour.

20th.—Continues to improve. There is no involuntary discharge of urine, nor can he void any excepting by the assistance of the catheter. Mild alvetic pills act favourably upon the bowels.

His convalescence was slow but regular, and he is now (August 1828,) returned to his usual state of health, excepting that he feels less strong than he was before his illness. He never allows the bladder to become so full as for any involuntary discharge to take place, or for the tumour to become perceptible. No voluntary power over the bladder has returned. Pain along the urethra is the indication of the necessity to introduce the catheter, and this generally occurs every five or six hours. He is able to walk about and use his accustomed exercise.

It is probable that some of your readers may feel surprise that the nature of this gentleman's complaint was not sooner detected. Without any attempt to dispute their penetration, or to justify my own want of it, I give the case just it occurred in practice, with the hope that it may prove useful to others. Late as the knowledge of the disease was obtained, it was a source of great satisfaction to me that it was procured in time to relieve the patient, instead of being discovered by a post-mortem examination—a period to which alone at one

time I looked for an explanation of the symptoms.

When the nature of an obscure disease has been unravelled, there is often but little difficulty in deciding upon the course that should have been pursued: but they who have been longest accustomed to medical practice, can best estimate the difficulties with which the path of a practitioner is beset in cases of an ambiguous kind, where a valuable life is at stake, and where the hopes and fears and interests of anxious relations are contributing to perplex his mind, and to increase his diffidence of his own judgment.—I am,

JOHN BISHOP ESTLIN,
Member of the Royal College of Surgeons, London, and of the Royal Medical Society, Edinburgh.

Bristol, August 16th, 1828.

From the Medico-Chirurgical Review.

AN ESSAY ON THE REMITTENT AND INTERMITTENT DISEASES, INCLUDING, GENERICALLY MARSH FEVER AND NEURALGIA; *comprising, under the former, various Anomalies, Obscurities, and Consequences; and, under a new Systematic View of the latter, treating of Tic Douloureux, Sciatica, Headach, Ophthalmia, Toothach, Palsy, and many other Modes and Consequences of this Generic Disease.* By JOHN MACCULLOCH, M.D. F.R.S. &c. &c. Physician in ordinary to His Royal Highness Prince Leopold of Saxe Cobourg. In two Volumes, 8vo. 1828.

In some late Numbers of this Journal, we have given a very full account of Dr. Macculloch's work on Malaria, as the cause of the various (at least variously denominated) diseases in the two volumes now before us. It will require three or four articles to convey any thing like an analytical delineation of these volumes; for, although it will be readily perceived that our learned author has a hobby-horse, on which he has quietly rode for more than 20 years past, pursuing the bent of his own inclinations, investigations, and meditations, yet we do not think that his speculations are all visionary, though some of them are probably seen through a coloured, if not a distorting medium. Dr. Macculloch is a great philosopher and logician—and he wishes to see all medical inquiries carried on according to the strict rules of philosophy and logic. That he is frequently a little ruffled in his temper, on finding that physicians have not generally conducted their investigations on these *beau-ideal* models, will be sufficiently apparent as we proceed. That he is sometimes pretty sharply critical—we had almost said *cynical*, will probably be inferred from the following passage in the preface to these two volumes. After telling us that science begins with conjecture and assertion—that its infancy is the reign of the imagination—that "physic is yet in that very infancy, wandering about its own fairy land"—that this science abounds in bad observation, in imaginary experience, "and even in positive *mala fides*"—"that the laws of philosophy and *logic* (what will Sir Gilbert Blane say to this?) have scarcely yet found their way into it," our author winds up with this terrible Philippic.

"The language of truth is simple and brief, but that is not the language of physic. Its words have meanings, and the same words have always the same meaning: but this is not the language of physic. The language of error is multitudinous, variable, vague and unsteady: and this is the language of physic. If there be a philosophical reader who doubts this, if there be a logician, a man accustomed to evidence, who has not read medical books, let him read even the most celebrated, and be satisfied."

These are hard words master! Perhaps, if the learned author had toiled, during the said 20 years, at the bedside of sickness, he would have been more merciful to his brethren. It is not in books alone that we see the most clashing testimonies, contradictions, and contrarieties. He who has seen most of human nature, and especially of human maladies, is most convinced of the existence of these contrarieties, and of the impossibility, in the present state of our knowledge, of reconciling them by means of philosophy and logic. We make allowance, therefore, for Dr. Macculloch, because we believe he has been in the habit of viewing things rather as they *should be*, than as they *are*. The philosopher forms an imaginary picture, in his mind, of what the order of things ought to be in this world—but when, like Parnel's hermit, he comes out among men, he discovers a terrible difference between the closet and the crowd! The disappointment of the worthy Doctor, at not finding physicians to be philosophers, brings up the rather ludicrous reminiscence of Sir Joseph Banks' disappointment, at finding certain little animals remain black, after undergoing the severe discipline of ebullition,

"Fleas are not lobsters, d—— their souls."

Dr. M. observes that—"he who is now the philosopher in physic, is also a Pythagoras, while he does not *perceive* it." We confess that we cannot clearly *perceive* the drift of this occult passage—nor that of the following:—"The physician is like Nebuchadnezzar—he dreams, and death is the sentence of him who cannot divine what that dream was." We humbly propine that, when the physician dreams, he also sleeps—and we have a notion, that Death is not always most active when the doctor nods. But, leaving all prefatory matters aside, we come to the philosophy and logic of the work. The first of these two volumes is entirely occupied with the subjects of remittent and intermittent fevers—dysentery—and cholera. The first of these topics will afford ample materials for this article.

ORDINARY REMITTENT OR MARSH FEVER.

Dr. M. must be an extremely discontented man. He sets out by informing us that, on no

disease in the whole circle, has he derived less information from books than on this. "He must labour with no small discrimination, who would, from medical works, extract any rational account of the immediate causes of the disease, or of the real condition of the system under it—who would discover any intelligible and consistent method of cure—who would even be always certain that it is of this disease, and not of contagious fever, that he is reading." What if we were to tell Dr. M., that the causes of this fever are not always precisely of the same kind—and that this observation might apply to the conditions of the system—and, consequently, to the methods of cure! Nay, we would have little hesitation in going a step farther, and saying, that Dr. M. himself would not always be able to discriminate between marsh fever, under particular circumstances, and contagious fever, if it do not actually take on a contagious character.

The object of our author, in this essay, is to class together some disorders which appear to him to have been misunderstood, as well as misplaced, in relation to their causes and characters—and, consequently, whose treatment has been erroneous. At the head of this class, as the offspring of malaria, stands remittent fever—the most important link in the philosophical chain—and the cause of many other diseases. Dr. M. properly observes, that the more perfect forms of diseases, as described in books, are much more rare, in nature, than the ill-defined forms, and, hence, the unreasoning, and, indeed, the inexperienced practitioner, is daily at a loss to name or classify the multitudinous forms that present themselves to his view. It is our author's object, therefore, in this work, chiefly to notice that which is obscure in itself, or least generally known, in marsh fever; thus making the essay a kind of supplement or addition to the systematic descriptions already existing.

Although there can be no reasonable doubts, that the general cause of remittent fever, in its perfect form, is the application of malaria; yet our author acknowledges that various other causes do produce it, as heat, errors of diet, fatigue, cold, mental anxiety, &c. "But every one of these is an accessory cause of many other disorders;" and if malaria be more copiously generated, or more widely diffused, than is usually suspected, it may still be the real productive cause, where the causes above-mentioned are only auxiliaries, though the only ones that are apparent. The subject of the malaria itself has been amply discussed in a former volume, and all that he can permit himself here to say is—"that he has attempted to prove that all the fevers of any moment, which are not produced by contagion, are the effects of malaria, very often, or, perhaps, very generally, overlooked." All other fevers, arising from other causes than malaria and contagion, are comparatively trifling in number or power. Our author recognises one exception to the rule respecting the cause of remittent fevers, namely, HABIT. It is well known that agues and remittents are sometimes reproduced, where the original cause cannot be in operation. It is probable, however, that, in all these relapses, some of the auxiliary causes have been exerting their influence on the constitution.

After some observations on the comparative susceptibilities of natives and strangers, in malarious countries, Dr. M. touches on the *time* which intervenes between the reception of the poison and the manifestation of its effects.

"If my own frequent observations show that fever may be induced within half an hour after exposure to malaria, and that a single inspiration, or the space of a very few seconds, is amply sufficient for the purpose, this is also an opinion most decidedly stated by many French and Italian physicians whose experience and acuteness will not be questioned. It is equally the opinion of other observers, not physicians, and, therefore, without the bias which might be suspected in such cases: of military, and chiefly of naval men, whose observations have been founded on the momentary and transitory effects of a breeze of wind, and especially of a land wind blowing off to sea. In France and in Italy, to confirm this, instances are known and recorded, of labourers dying instantaneously from merely sitting or lying down on the ground, and of others who from looking into a ditch or drain, have been struck dead by that poison which, of course in a minor degree, would have merely produced a fever. Lind, also, whose authority stands high, describes the instant seizure with nausea and delirium, as many others have done; so that respecting this part of the question there needs be no dispute."

That the miasmal poison may, in certain states of uncommon concentration, be capable of producing an instantaneous, or even fatal effect on the nervous system, we do not deny; but that a regular fever is so quickly produced, we are inclined to doubt. It seems, in general, to require a certain period for concoction in the system, before fever is evolved. The extreme length of the interval next engages our author's attention, and after a minute examination of evidence, he appears to conclude, that 15 or 20 days from the utmost limit—perhaps more than the limit. Thus a ship's company became affected with remittent fever, on the coast of Africa—the ship put to sea, but some of the men continued to fall ill with the fever till the 20th day, after which no more cases occurred. Here, too, Dr. M. says there may have been fallacy. There may have existed a foul hold, from whence the febrific miasm continued to ascend—or the fever might have become contagious. "A remittent will become, or perhaps produce, in any given individual, a contagious typhus, under confinement." But independent of this hybrid, or contingent disease, we think there are sufficient facts on record to prove, that miasmal fever may take place after a longer absence from the source of the miasm.

"Many physicians or surgeons, both English and French, have said, that even after six

months, many soldiers who had been at Walcheren and had escaped the fever there, were seized with the same disease in other countries; asserting also, that the poison had remained during the time dormant in the constitution. Pym is one of those who thinks thus, and so I imagine does Blane; while Bancroft believes, that the intermittents of spring are the produce of malaria received in the previous autumn. Baumes, resembling Lind, limits the term to fourteen days; but Ferrus, coinciding with the former, relates in proof, a case of a soldier who having escaped at Walcheren, was affected with this fever six months after, on the Niemen; as there is also a case quoted in evidence, where an English regiment became attacked in the same circumstances in England, after eight months."

An objection might be readily raised to the latter fact, that a fever produced *on* the banks of the Niemen, may be produced *by* the banks of the same river. And so an English regiment serving in Spain, after the expedition to Walcheren, might very readily pick up some malaria in the former place, without any necessity for carrying it from the banks of the Scheldt. In a physiological point of view, it is difficult to conceive how a poison of this, or of any other nature, should lie dormant so long in the system. It is easier to conceive the reception of a new poison, and the production of a new disease.

Dr. M. very properly passes over, by a reference to a host of authors, the symptoms of remittent fever, both in its simple and complicated forms—together with the almost endless modifications produced by climate, epidemic influence, and various other causes, including differences in the miasma itself.

"But I may mention, that while an undue, and apparently a morbid secretion of bile, is the most conspicuous and common local affection, producing sometimes what is emphatically called the yellow fever, so the brain and other organs, and above all, the stomach and the bowels, are often found affected by inflammations, modifying materially the symptoms, and also demanding important modifications in the practice."

In referring to systematic writers for general descriptions and local peculiarites, Dr. M. *feels* himself compelled to notice that characteristic symptom, whence the disease derives its *name*. This is the remission or diminution of the intensity of the febrile symptoms, occurring once in the 24 hours, however variable in the period of its arrival, its duration, and degree. Sometimes this remission is so slight as not to be perceived, though errors are often committed here, especially when the remission occurs in the night, or in the absence of the medical practitioner. For our own parts, we can hardly say, that we have ever seen a decidedly continuous fever. There is an evening exacerbation and morning remission in almost every case. It is hardly necessary to say, that it is by a gradual prolongation of this interval of remission, that the so called continued fever of malaria becomes an intermittent—and this leads our author to speak of the terminations of the disease. There is, no doubt, a disposition in most fevers to resolve on certain days termed critical, though these are now but little observed.

"Three weeks may probably, however, include the much greater number of terminanations in recovery, when the disease submits to the law of the critical days; while it is not uncommonly protracted to six weeks; and even in cases where its extreme mildness might have led us to expect an earlier solution."

Where this law of crisis does not seem to exist, we can assign no period for the recovery or the fatal termination. The favourable termination is often perfect, in all climates; but it not unfrequently changes to an intermittent, easily, in general, removed by proper remedies—but often peculiarly inveterate and indomitable, as those who have witnessed Walcheren, some parts of China, Greece, Italy, Spain, Moldavia, and many other countries, can testify.

But remittent fever not only terminates in ague—it frequently produces, or ends in, "the local and painful affections of the nerves which may be ranked under the general term NEURALGIA." After alluding to the production of "paralytic affections," by remittent fever—or the termination of this fever in such affections, Dr. M. makes the following observations, which bear on a point of pathology, or etiology, recently mooted in one of our medical societies.

"That marsh fever does act directly, itself, or its generating poison, on the nervous system, is proved by the state of apoplexy or profound coma with which the attack is sometimes ushered in; a fact common in Italy, and known by the name of febbre larvata; though, in this case, mere intermittent may also be the supervening disease, instead of remittent. That, in these fevers, the affections of this nature have been attributed to local diseases of the brain, I know; and such events may doubtless occur. But this does not explain the cases in question, where the affection of the brain is instantaneous, following directly the application of the poison, even before fever is produced, and resembling that which occurs from the application of other poisons, whether to the lungs or the stomach.

"Farther, as it is the effect of Malaria to produce the local affections of particular and single nerves, either with supervening or present palsy, or without either, while the brain is not affected, and while no local inflammation or other disease of that organ can be supposed to exist, from there having been no previous fever, it is plain that Malaria does exert a power of some kind on the nervous system directly; on the whole, or on more or fewer of its parts, even to a single point in the minutest nerve."

Thus the larger paralytic affections, as hemiplegia, or paralysis of a leg or arm, consequences of an intermittent or remittent, may, he thinks, result from "direct action on the

nervous system," and, in this way, he also thinks, may be explained that diminution of intellectual power, proceeding often to perfect idiotism, which sometimes follows long-continued intermittents. Dr. M. admits, however, (what we think is the more probable solution,) that there may, in such cases, be organic disease in the brain itself, the product of local inflammation there. He does not consider this, indeed, as the more common source of the paralytic or cerebral affection.

"But if malaria does produce direct apoplexy, as it also often brings on a comatose state which is exceedingly durable, both in remittent and intermittent, and if also it produces, not only local and similar effects on single nerves, but complete hemiplegia, it will be most necessary to inquire whether some of the cases of paraplegia or other palsy, especially as occurring in certain climates and in campaigns, are not instances of the same nature; since, whether our practice in such a disorder should be different or not from the treatment of palsy produced from simple cold, the philosophy of physic cannot fail to be improved by discovering causes and assigning distinctions."

The subject of visceral or glandular disease is next taken up. Most authors have looked upon these as the effects of the fever which preceded them—though a few writers have considered the visceral affection as the primary, and the fever as the secondary link in the morbid chain. Dr. M. appears to think that both hypotheses may be occasionally right. Thus, some physicians have maintained that the hepatatis of India is the direct effect of a morbific miasm—and, if so, other glands and organs may be the primary seat of action when malaria is received.

"Thus also, through France, Italy, Sicily, every where, it is common to find, not merely single instances, but a whole population, suffering from glandular diseases in their worst forms; while no fever is present, and while also, in many cases, it seems to be ascertained that no fever has preceded, or that there has at least been no severe remittent or intermittent as the cause."

Our author concludes this chapter with an expression of his disapprobation of those wild theorists who disbelieve in contagion altogether. No doubt the great mass of fevers in this country are devoid of contagious character, because comfort and cleanliness are observed—"but to assert that there is not such a thing as contagious fever, is to discredit evidence as numerous and incontrovertible as science or human affairs have ever produced." The fevers not contagious, he thinks, must fairly be ascribed to malaria.

The second chapter is on the chronic or relapsing, and obscure or anomalous remittent fever. This, Dr. M. observes, is a modification which is often but little noticed, while it is a source of great distress.

"If this peculiar variety is sometimes sufficiently severe and marked to be esteemed a fever, it is far from uncommon for it to be so slight as to pass for hectic, for what is called debility (a term without meaning) or for ill health, or delicate health, terms equally convenient to cloak ignorance; while not unfrequently also, it is characterized by the no less convenient phrase nervous, or even brings on the unlucky patient the charges of hypochondriasis or affectation."

To be more particular; there is, says Dr. M. a fever not uncommon among us, to which the popular name of nervous fever is not inaptly applied, when its symptoms are not severe. Dr. Cullen was unpardonable, he says, in confounding this with contagious fever, under the name of typhus mitior. It is not contagious, nor produced by contagion, as far as his own observations have gone. "Durability, or the property of prolongation, seems to be a peculiar character of marsh fever, under all its forms; and, until an unequivocal case of contagious fever, thus mild and thus durable, is produced, I must continue to believe, that all long-continued or often-relapsing fevers belong to the disorder under consideration." The same remarks apply to what Cullen denominates synocha or synochus. By this, however, Dr. M. does not mean to deny that there can be such a thing as a pure inflammatory fever, produced by cold, or the other causes usually assigned; but he believes that the disorder so called is very frequently "a fever of the remittent family, and produced by the same causes." Many plausible arguments are adduced in favour of this position, for which we must refer to the work itself. If this "low fever"—"fever on the spirits"—"fever on the nerves," (by all which names it is known,) be not a modification of remittent, our author knows not how or where it is to be classed. Physicians must erect a new genus for it—since it does not belong to one or other of the two leading classes.

"And it is far from unimportant that this point should be clearly understood; as it is only thus that our practice can be justly regulated: while it is most certain, that by mistaking it for other diseases, the sufferings of the patients have often been, and are daily and every where, materially aggravated And if the cause, the original one, be malaria, as in the case of acknowledged remittent, whatever the causes of the relapses may be, we thus acquire the means of prevention; of which, as long as we mistake its nature, we cannot avail ourselves."

Among other arguments in support of the said position, Dr. M. adduces this one, that the disorder in question is among those habitual complaints included under the vague term "ILL HEALTH," which are the produce of low and wet situations, or of some of the soils formerly described as productive of malaria.

"Another argument is, that its relation to the marked or severe and terminating remittent, in slenderness of symptoms and in the frequency of its recurrence, is precisely that which intermittent, equally slender and equally returning, bears to a limited and severe intermitting fever, while I may lastly add, as a

proof of its cause and return, that if it is especially subject to relapses in low and wet situations, as well as indebted for its very existence to those, so it is best cured, and especially when relapsing, or repeated, or chronic, by change of air; that is, by change to a drier air as it is usually termed, or, what is the fact, by removal from its causes."

Dr. M. thinks it probable that the disease in question, is that which Dr. Haygarth has described in the College Transactions, as inexplicable—"a peculiar state of permanent debility, enduring even for years, and without very marked disease of any kind." Unless it be attributable to the abuse of purgation and bleeding, Dr. M. is unable to account for this complaint, except by assigning some morbific exhalation from the earth as its cause. We must now allow our ingenious and indefatigable author an opportunity of describing the disease under consideration.

"This disorder may be found, and not unfrequently, with scarcely any marked symptom except mere muscular weakness; a debility on any attempt at exertion, which seems unaccountable, inasmuch as it occurs in persons, even in youth, and apparently strong, and is not very obviously accompanied by any proper febrile symptoms. At times, not even the appetite seems affected; and here, almost necessarily, the result is, to suspect the state of the patient's mind, or his moral dispositions, rather than his health; to suppose, for example, as I have often seen, that a soldier is 'shamming,' that an opulent female is indolent or affected, or a studious or professional man hypochondriacal.

"Yet, let an acute physician watch this disease, and he will be convinced that it is a disease, and moreover a fever. It commences and terminates like the remittent when best marked; and when it appears to be prolonged for months or years, as is sometimes the case, it will be easy to see that it has had intervals of cure, generally of self-cure, and relapse; and that, to each relapse, there is a period of weeks, not very uncommonly of six, while the intervals vary from one or two to any given number. Further, either the patient or the physician, or both, must be very inattentive if they do not discover that the paroxysm of extreme debility is fixed; that it is, in fact, a paroxysm, let its length be what it may, and that there is a diurnal period when it diminishes, or where the patient, who, possibly, could not stand, on getting up in the morning, is enabled to exert, and even to enjoy himself at night.

"Hence, as to some cases, at least, the truth of, as well as the reason for, a very common remark, that midnight is the nervous patient's holiday; though there are unquestionably many cases of nervous affection, and even of periodical returns and intermissions in this complicated class of disorders, which do not appertain to a remittent type of fever, or perhaps to any fever. The particular case here quoted, is one, of course, where the paroxysm attacks in the morning and the remission is at night; but while the periods are necessarily various, so are the results, as to the complaints, appearances, or sufferings of the patient. I shall presently trace some others of the more marked of these modifications."

Between the above and the unequivocal forms of remittent fever, there are, of course, a great number of shades, in which the obscure symptoms gradually multiply and become more conspicuous, indicating, as they advance, a more severe disease. It would not be difficult, Dr. M. adds, for any attentive physician, in tolerably extensive practice, "to collect a series of cases rising in exact gradation, from the simplest debility to the most perfect form of remittent fever." The appetite, though sometimes apparently unaffected, is generally irregular or capricious—vanishing in the paroxysms, and returning in the intervals. The tongue and the secretions will usually show indications of disorder, but none peculiar or pathognomonic. These disorders of the primæ viæ Dr. M. looks upon, of course, as *effects*, though they are generally viewed as *causes* of disease.

"In the whole catalogue of ordinary practical errors, I know few indeed more common than that which views a sluggish state of the bowels as a primary disease, sometimes also a consequence of theoretic disorders of the liver, instead of considering it what it really often is, the produce of a febrile state, belonging, either to this fever or to some other initiative and similar cause. Nor is it difficult to account for this error, vulgar as it is common; since it is the consequence, partly, of seeing, in a disorder, nothing but obvious symptoms, and partly of that empirical practice for which England is so celebrated, and which, while it tends to blind the judgment, can, from its facility, be conducted by any one."

This will not be a very palatable doctrine to the routine practitioner, and especially to the disciples of Abernethy, who have had halcyon days of it, for many years past, in consequence of the great simplicity, or rather uniformity to which practice was reduced, as a result of the doctrine that made but one cause for all disorders. We are disposed to think that the moral world will not *alone* suffer from those disturbances occasioned by the "march of intellect"—the physical world, or at least the world of physic, will come in for a share of revolution.

But to return. Our author has no hesitation in averring, that fevers of malarious origin are a very general cause of the chronic and common derangements of the digestive organs, to which so much attention has, of late years, been paid. In respect to the state of the pulse, Dr. M. justly observes that, "there are persons who cannot conceive a fever without an accelerated pulse; whereas, even in severe cases of remittent, the pulse often gives no indications of any disorder, or the very reverse of what such practitioners would have anticipated." The fact is, that in these forms of complaint, the pulse may be natural during the greater part of the day, but considerably

accelerated for an hour or two, during the obscure paroxysm which marks the disorder. This often, perhaps generally, takes place in the evening or in the night, and consequently escapes the observation of the medical practitioner.

"The diseased state of the mind may, however, exist at two distinct periods of the paroxysm, and under two different states of the pulse. Under the accelerated or contracted one, it is a state of peevishness or irritability, attended by the feeling of despondency or not: or it is a modification of the great leading passion anger, which, together with fear, the equally inclusive and principal passion, forms those deranged states of mind appertaining to hypochondriasis, which appear under so many modes and modifications. And if under the full and slow pulse, it is commonly simple or passive despondency, or, in extreme cases, despair, so if the opposed condition or passion, irritability or peevishness, belongs to the accelerated pulse, that, in similarly extreme cases, may amount to anger, or to a tendency to that fundamental passion, easily excited by trivial causes; not seldom, difficult to restrain, even when no external cause is applied, or proceeding to causeless conduct, even in solitude, unaccountable to the patient himself."

This symptom of mental derangement may be the only one apparent to a bad observer. Despair and fear, especially the latter, appertain peculiarly to the class of marsh fevers—so much so that, in some parts of the Mediterranean, when these fevers are endemic, the only name by which they are known to the common people, is *scanto*—fear or fright. This circumstance is adduced by our author as corroborating his views of the connexion between hypochondriasis and malaria. He cautions the reader, however, against concluding that he is endeavouring to draw a sweeping conclusion that all cases of hypochondriasis are dependent originally on miasmal disorders. Dr. M. makes many judicious and ingenious observations on the effects of this slow or obscure remittent fever on the intellectual faculties, but we cannot so far disentangle them of the monstrous load of verbiage under which they lie buried, as to exhibit them here. The reader must plod, and labour, and unravel for himself. The two principal mental conditions induced in this way, are torpidity and excitement.

"The state of torpidity or inability accompanies that condition which must be considered as the cold stage, or which is the commencement of the diurnal paroxysm; being noticed, of course, only when the attack commences in the day and in the hours of labour, and therefore often passing without remark. And in every fever, this is the period of peculiar mental inability; the one observation confirming the other.

"The period of excitement, or of increased, if of hurried mental power, on the other hand, is the hot fit, or that which is here its substitute; a period of partial delirium: and here also, that condition of mind which is useful in moderation, is illustrated by the other, or by that excess which causes the imagination to run wild."

Other bodily symptoms are next investigated by Dr. M. Headach, though not always present, is much more common in women than in men. In the former, debility and headach often constitute the whole of the obvious disease. "With these symptoms alone, or apparently so, the disorder will sometimes run a course of six weeks, and with such severity as to confine the patient to bed." To the above symptoms may be added fits of restlessness or lassitude—occasional pains in the limbs—drowsiness, sometimes irresistible, and very remarkable in the chronic remittents of Italy and other malarious countries—natural sleep disturbed or irregular, being absent in the early part of the night, and coming on at the time when the patient ought to get up. The following is a curious instance of this class of disorders haunting an individual for 30 years!

"In this, the patient had, for thirty years, been subject to nearly all the diseases in rotation which I here rank under those of malaria, namely, to remittent, to intermittent, and to almost every known variety of neuralgia; having apparently acquired the incurable habit of these disorders at an early period of life. In several long intervals among those more marked ailments, the same person had also been affected, for long periods, with simple coma or drowsiness, and further with nocturnal awaking in the state of partial delirium just described: and it was his invariable remark, that the hour of awaking in this manner, was always precisely the same as that which marked the paroxysms of the intermittent and those of the neuralgia, indicating their joint dependence on one cause and one habit; while that hour scarcely ever had varied by many minutes during the whole of his life of disease."

A disordered state of the digestive organs is so inseparably connected with the complaint in question, that our author seems doubtful whether to consider it a constituent part, or a consequence of the general malady resulting from malaria.

"Let physicians watch their own cases of this disease with this new light, and they will scarcely fail to find evidences of their own which will be much more satisfactory. And they will recollect also, when they reflect on their practice, how often they have found dyspepsia periodical under diurnal returns of various kinds, how often periodical and dependent on seasons, how often they have seen it cured by merely altering the hours of eating, how often by bark, or by arsenic, or by the other tonics that cure the remittent and intermittent diseases; and how often by change of air, and also by mental affections, or causes operating on the imagination, such, among others, as the change of physicians; all of them remedies for the intermitting diseases in question."

The author observes that, in the various works which have been written on dyspepsia,

he has found no author who has taken this view of the complaint—who has looked to malaria as the cause. It is true that Dr. M. is original in making miasma so prominent a case of dyspepsia, but if he will examine the works of authors again, he will find that "*impure air*" —which surely must be synonymous with *malaria*, has not been overlooked in the etiology of dyspepsia.* But malaria is only one of the thousand *physical* causes which disorder the digestive functions—and for one physical cause of this disorder, there are, at least, two moral causes! It is but fair to say that Dr. M. considers the catalogue of real causes of dyspepsia too long for insertion in his work.

"Of the very numerous real causes of this common disease, I cannot pretend to give even a catalogue, as that would be to transgress my plan; nor could I therefore enter upon any comparative view of the predominance of these several causes, or attempt to suggest what place the one here proposed may deserve among them."

Hysteria is considered as one of the symptoms attending this class of complaints—"though rather an incident than a portion of the disease." In some of the more strongly marked forms, however, of the low remittent, "the occurrence of the hysterical paroxysm, which is rarely more than a fit of crying, is commonly as regularly periodical as any other portion of the disease." Dr. M. has generally observed that it attends the subsidence of the pulse, or, in other words, the termination of the excitement. It is curious, that where the disease altogether is so slight, that the patient, if courageous, bears up against it, and does not complain, this hysterical weeping or feeling, is the only part of the disease which cannot be resisted. "A long-continued attempt at exertion, or at concealment which is exertion, is, in such cases, generally followed be a hysterical affection unusually severe." We coincide with Dr. M. in believing that most of those recorded cases of *periodical* hysteria belong to this class.

"It is one of the effects of this remittent, particularly when chronic or habitual, to produce those general derangements of the entire health which it would be tedious and equally useless to enumerate, as they are familiar to every one; while, with that, the temper, and even the moral character, as it may be considered, become also permanently or habitually injured."

Among the symptoms or consequences of the obscure malarious disorder now under consideration, we must not pass unnoticed those menstrual errors generally presented in the shape of amenorrhœa and dysmenorrhœa, the former particularly being often attended by a chlorosis that is mistaken for an original disease. We agree with our author that these complaints of the uterine system are "much more commonly the results of some derangement of the health than the sources of that ill health by which they are acompanied."

Dr. M. has hazarded an opinion, that the climacteric disease, described by Sir Henry Halford, is a form of this chronic and obscure malarious malady. His arguments and illustrations will be found between page 105 and 108.

The original cause of this class of complaints is, of course, the malaria so ably investigated in a preceding volume.

"Besides this original cause of all the evil, however, numerous other causes inducing debility will reproduce the relapse, and thus tend to perpetuate the disease; and the more readily as it is a more confirmed habit. Such are ordinary cold, fatigue, intoxication, bleeding, the excessive use of saline purgatives, mental affections, with others unnecessary to name; all of them equally efficacious in recalling the returns of a chronic tertian or quartan. Of all those causes, I would particularly notice here the use of purging, and mental affections; as, respecting the other, no one doubts much, and as they are commonly avoided. That what is called 'a course of salts,' will very often reproduce an attack, I have abundant proof; and it is especially necessary to notice this, since it is commonly resorted to as a remedy for the imagined diseases to which the symptoms of this fever equally belong. Hence the frequently injurious effects of that fashionable folly, the frequenting of mineral wells; a practice resorted to by presumptuous patients, or by vulgar practitioners, as if it must be universally salutary, and was incapable of doing harm. And the common error in this case, as it is the especial cause of this erroneous and pernicious practice, is the mistaking the derangements which I formerly noticed for dyspepsia, as it is called, or liver complaints, or whatever else, under this received phraseology which is now so current; while the empirical practice to which I then alluded is applied without examination."

The existence of local pains in this class of complaints often induces to blood-letting, which generally aggravates the symptoms it was meant to relieve, besides protracting their duration.

"As to the influence of mental affections, it is rather a matter of curiosity than use, as the injurious occurrence of these can scarcely be guarded against; but it is, in the philosophy of physic, an interesting fact to observe, how instantaneously sudden grief, fear, disappointment, or other strongly depressing passions, will bring on that relapse which will generally run the same course as all the preceding."

Dr. M. remarks that, although this obvious

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* While enumerating the *causes* of dyspepsia, Dr. Johnson makes the following remark: —"Where air, imbued with millions of *miasmata*, exhaled from every thing in the animal, vegetable, and mineral kingdoms, is breathed, swallowed, and kept in contact with the skin, the effects are conspicuous in the sallow complexions, puny or capricious appetites, and *imperfect digestion* of the inhabitants."—*5th Edition, p.* 53.

disease has been so much overlooked, he has no doubt that it will shortly appear very common, now that it is distinctly pointed out. England has, till very lately, been so free from intermittent fevers (which form the readiest road to the analysis of the present disease) that but a few, proportionally, of medical men, have seen an ague at all. Hence, too, their attention has been but little directed to the investigation of malaria, as its source. There can be no doubt also, that in numerous instances, fevers resulting from miasmata have been confounded with typhus.

PROXIMATE CAUSE OF MARSH FEVER.

Our satirical author remarks that, it would be well if the professor, who spends months in exciting the wonder and applause of a juvenile audience with phraseology which he does not himself understand, would substitute for all this waste of words and time, the confession of his own ignorance. "For never yet has philosophy thriven by dressing up fiction and vain speculation in the garb of truth and sense. Physic knows not how the poison of fever acts, nor on what it acts—what are the preliminary effects which produce the symptoms that are obvious to our senses." We cannot, he says, even conjecture why these actions should cease—why they should be renewed—or why they should cease to be renewed. But, although Dr. M. can offer no theory of his own, he takes leave to criticise those of others. Dr. Clutterbuck's doctrine of cerebral inflammation he passes over. The action of malaria, say the French, is stimulant, and the symptoms of debility which succeed, are the effects of previous exhaustion. On this the following remark is made.

"If indeed the action of prussic acid, or lightning, or a cannon shot, is sthenic, then the assertion will not be disputed: and thus, he who, under the action of malaria, falls down instantaneously with apoplexy, has died of over-excitement."

The fever of marshes, say others, is a gastro-enterite, or inflammation of the mucous membrane of the stomach or bowels—and every other effect and symptom is sympathetic or consequential—and the success of the practice is said to be confirmative of the theory. Dr. M. loudly protests against this exclusive theory; but he only adduces those arguments, which others, as well as ourselves, have repeatedly brought forward against the *abuse* of the doctrine. "Whatever dissections have taught, they have not taught us the cause of marsh fever." "They have taught us, that certain effects take place occasionally:—that is their use." The inflammations are effects not causes. Dr. M. admits the periodicity of inflammation in certain agues and neuralgias, but contends that these inflammations are not common, but specific inflammations; *sui generis*—and the circumstance of their being cured by bark and arsenic, proves them to be so. It is hardly necessary, after these observations, to say that our author gives up, in despair, any attempt at a proximate cause of the disease under review.

Treatment.—Dr. M. sets out by renouncing all idea of the treatment of remittent fever, as it appears in tropical climates, or in aggravated forms in any climate. "The most opposite opinions have been entertained, and the most opposite practices followed. As happens in tetanus, all these modes have failed—all these modes have seemed to succeed."

"The conclusion of him who knows nothing of physic, will probably be that the imagined remedies have had no concern in the cures, though he will scarcely conclude that they have had none in the ill success; while a fatalist in medicine, as fatalists there are, will perhaps determine that the efforts of the physician are nugatory as to either event."

Dr. M. touches on the administration of emetics, at the very beginning of a remittent, with the view of "putting it out." He does not seem to have much confidence in such a procedure, and thinks that it often produces or aggravates that peculiar inflammation of the stomach, which accompanies the fever. "How often death has been the result of such emetics, given improperly, or pushed too far, is well known." The general antiphlogistic measures necessary in the early stages of fever are passed over, as universally agreed upon. In respect to purgatives, Dr. M. makes some judicious observations. Where bleeding and antiphlogistic measures are proper, the saline purgatives, by producing watery motions, lessen the whole circulating mass—and are thus useful; but where the disease will not bear, or does not require sanguineous depletion, the said saline purgatives are improper, as tending to induce debility. Those resinous purgatives, however, which act locally, as wine, and merely promote the natural evacuations, are beneficial in all fevers, and almost all stages of fevers. On calomel, Dr. M. also makes some comments. In the remittents of hot climates, he has no reason to doubt its efficacy—in the milder fevers of our own country, he does not suppose that it exerts any specific influence "beyond such as is derived from its power over the biliary system." In regard to the chronic or relapsing variety, to which our author appears to have paid much attention, the following observations are deserving of notice, especially as they come from "one of those physicians who consider that this medicine (calomel) has been greatly abused."

"This remark is, that in the relapsing disorder, even if the attack should occur at the end of winter, in patients free from all suspicion of deranged liver or biliary affections, and when not the slightest indications of these can be traced, and when, further, the relapse may be the twentieth or fortieth to that patient, the operation of calomel is to produce obvious effects, which, if I need not specify them, physicians know well to be those which never occur except under derangement of this secretion. And at the same time, what is the important point here, it will be found that

after every such effect of the medicine, the force of the disease diminishes, and that whenever the natural secretions recur, that particular relapse is about to terminate. Hence, therefore, I am led to consider, that even where it is least suspected, and indeed not to be believed present, there is often, in the chronic relapses, a derangement of the biliary functions; and that calomel, being the remedy for these, is apparently a remedy which cuts short or cures that relapse."

The use of the medicine is recommended as long as it is found to produce the evacuation of morbid bile:—when this disappears, the calomel becomes injurious.

On sudorifics, opium, cold affusion, and diet, some observations are made that do not require notice. Dr. M. very properly remarks, that food should never be given, however light, except in the intermissions or remissions. At all other times, even in the most mild and chronic remittent, it does much mischief by ruffling or irritating the stomach.

The grand point or question is that of administering tonics—and more especially bark, in this class of diseases. Dr. M. acknowledges that this is "a complicated question." There can be no doubt that there are instances where bark fails, or even aggravates the disease, even where no inflammatory symptoms are present. But, on the other hand, there are far more instances on record, where bark has cured the disease, during the actual and unequivocal existence of inflammatory action.

"It is as painful to a writer to leave his readers in suspense on points so essential as it would be presumptuous to decide; yet it may be suggested, that if, as will hereafter appear, the inflammatory affections of remittent are of a peculiar character, and not proper phlegmasiæ, and if certain visible and demonstrable ones are actually cured by this remedy and aggravated by evacuants, the question will not improbably be decided in favour of those who recommend it in all cases; and it may not be difficult then to discover that prejudice or incorrect observation will explain that testimony against it which has been thought to be derived from experience."

No doubt exists as to the utility of bark, where there are intermissions—or even pretty fair remissions, with general symptoms of debility. The very circumstance of there being a tendency to remission or intermission in a fever, is in favour of the utility of tonics, when not strongly contra-indicated by local phlogosis.

On the subject of wine, Dr. M. entertains some eccentric, or at least heterodox opinions. It is proved, says he, that "wine increases inflammation, when existing, or produces a tendency to it in healthy subjects, or in diseased ones?" He asks whether these opinions are grounded on observation, or only hereditary dogmas "established no one knows why, and followed because they have been followed."

"But, granting that there are cases of inflammation, or a species of inflammation which wine would increase, physicians know full well that they are utterly ignorant of the real distinctions among inflammations which, to the sense or the eye, may appear the same; and that while there are some kinds or varieties which are to be cured by stimulants both local and general, as I shall hereafter show very fully, so are there inflammations, and apparently inflammatory states of the entire system with increase of circulation, where wine is a remedy instead of being injurious. Nor does it appear that the habitual use of wine produces a tendency to inflammation in healthy subjects; since it is notorious that among water-drinkers, the diseases of active inflammation are most frequent and require the most energetic treatment."

The remarks on blood-letting need not detain us. In robust subjects—at the first attack of the disease—the loss of blood is often useful in reducing that activity of the circulation, or that vigour generally, which renders the first portion of the paroxysm severe. When, also, there is unequivocal evidence of topical inflammation, we ought to have recourse to topical depletion. But, we need hardly say that Dr. M. is not among those physicians or surgeons who conceive that fever can be "put out" by venesection, however decisive, and however early employed,

"Were I indeed to indulge in that violence of generalization so usual with physicians, and so much too prevalent among those who undertake to point out or review a system of practice, it would be to assert that it were better that blood-letting should be utterly abolished in this fever, than that it should hold a place so egregiously abused."

From VENESECTION Dr. M. makes a rapid transition to WINE, the use of which in fever, says he, is "too often made a question of fashion and temper, rather than of rational and *sober* inquiry." How can we expect *sober* inquiry where wine is the topic? The following short quotation is all that is necessary on this point.

"That its singular combination of stimulant and sedative powers renders it one of the most convenient of the remedies generally classed under the vague term of tonics, seems to have been established by experience that can hardly be disputed; and the most determined theorist can scarcely deny to himself, that he has gained decided advantages from its use in the low or later stages of fever, and that it has often appeared to him the means of at least supporting the patient to a favourable termination."

Dr. M. after some remarks on subordinate agents in the treatment of fever, comes to the management of those milder varieties which he has described at such length in the volume before us. The following sarcastic observation is not destitute of foundation in truth—nor is it inapplicable to the *routine* practice of the day.

"A sweeping conclusion as to the ordinary simple fever of this character, would be, that it requires no remedies at all; and most assuredly, it is far better left to its own opera-

tions, or to nature, as the phrase is, than that it should be tampered with by intermeddling and routine practitioners. There is little good to be done by remedies; but it is not so as to the harm. Left to itself, its periods proceed in a very orderly manner to a favourable conclusion; but it is rarely so when a busy or active practice interferes. What the evils to be produced may be, can so easily be concluded from the preceding remarks, that I need not detail them."

A great deal of this routine and injurious POLYPHARMACY depends more on the wretched and distracted *system* of medical practice and education in this country, than on erroneous views of the disease. The medical man's *time* is his only fortune. Chronic diseases occupy far more of this valuable property than acute diseases; and as the general practitioner is not allowed to charge for his visits and his *advice*, he must necessarily send medicine in such proportion as will remunerate him for his *time*. Even when the physician or surgeon is called in, he must, as a matter of conscience, prescribe in the manner that repays the general practitioner for his attendance. He cannot, and indeed he ought not, to do otherwise —for, let it be remembered, that we are not to ruin ourselves for the sake of that public which persists in withholding from the profession the *proper mode* of remuneration. A patient will pay *only* for medicine—he considers the skill which directs that medicine as not deserving of any pecuniary remuneration. In God's name, then, let him have medicine—usque ad nauseam! The perversity of human nature, on this point, is really astonishing. The conscientious medical man, when he does not see clearly his way, and where no evident indication is to be fulfilled, would prescribe mere placebos—the common saline draught or camphor julep, which can do no harm, and by which he is remunerated for watching the disease. But the public is now got too enlightened to be put off with placebos. They must, forsooth, have medicines that will produce some ostensible *effect*, or no value is attached to the prescription! Assafœtida and camphor wont do. No! They must have something that will purge them, sweat them, make them sick, give or take away the appetite—in short, something that does violence to the constitution, or else the physician is an old woman, and the surgeon is picking their pockets by cramming down their throats a quantity of medicine, "which does them no good." Every medical man in *actual* practice —that is, who does not practise in the garret, with the gray-goose quill—can vouch for the truth of these observations. The natural and the inevitable consequence of such a state of things is, that all classes of medical practitioners are led into a system of prescribing *active* medicines, *in chronic diseases*, instead of managing the said complaints by a system of dietetics and general hygiene, that would be of far more use than medicine. Nothing but a general and consentaneous impulse and co-operation among medical practitioners can effect a change in this system, *at present;* though we are confident that reason and common sense will, sooner or later, correct the evil. A common *consent* or co-operation among medical men can hardly be expected, for some time to come, since the prevailing mania of the day is any thing but *concord*. Into whatever circle of society a medical man now goes, the common remark which he hears is—"what a *row* there is among the doctors!" In short, the medical profession is becoming the bye-word of derision among all the more enlightened classes, and we predict that the day is not far distant, when they will go in "sack-cloth and ashes," for their stupidity in being led away from the dignified paths of science into the arena of scurrility and personal abuse. But to return from this digression.

Dr. M. does not mean to say that the disorder in question should be absolutely left to itself—he only cautions against heroic remedies. *Quietude* is considered an important measure.

"Thus when the whole visible paroxysm is nothing but a muscular debility which, with care and rest, would have been limited to a few hours, an exertion through that time will not only protract it through a considerable interval, but also produce other symptoms and greater inconveniences: such as, increase of pulse, headach, additional loss of appetite, and an augmented sense of general suffering. The same consequences also follow from mental exertion under the same circumstances; and if I need not repeat what I formerly said respecting sleep, it is plain that the reasoning is the same. In all these cases, that indulgence to the feelings or caprices of the patient which humanity ought to dictate, is also correct medical practice; much too often, however, controlled by the ascetic principle on one side, and by that of what, for want of a better term, I may call tyranny on the other."

The bowels should be kept as near a state of nature as possible, avoiding purgation, except where there are evident indications of morbid secretions or biliary derangement. "In this moderate fever, also, if wine is not absolutely necessary, it is always convenient or useful." There is one state, however, where active practice is necessary. It is by no means unusual for this anomalous disorder of health to take on a decidedly intermittent form, and then bark or arsenic is necessary, and is generally exhibited at once—thus terminating in a few days, a series of teasing symptoms, or a condition of dubious convalescence, which might otherwise have dragged on for a long time. It is in this class of disorders that a sudden cure is often effected by change of habits, or rather change of air, especially where the original cause is malaria. Dr. M. asserts, and probably with some foundation in truth, that the routine practitioner not unfrequently hits on the real cure, without knowing what the disease is; and, while he is prescribing for mere debility, which is only a symptom or consequence of the disease, he gives the patient a certain number of bark draughts, for recruiting his strength, and

this medicine strikes at the root of the disease, which is a veritable though obscure *remittent*. After giving vent to a severe philippic against writers and practitioners who have failed to remark (though we beg to say that they have not all failed to remark) the frequent termination of common continued fevers by an intermittent form, Dr. M. observes as follows:

"And if this particular fact, or the subsidence of a continous fever to health through the intervention of an intermittent type, occurs very commonly in even the minor fevers, be their duration but a few days, or even one or two, then will it become additionally probable that even these fevers, be their technical names, or their imaginary causes, what they may, are dependent on the same cause as that which produces intermittents, or that they are true marsh or remittent fevers; since this is one of the essential and remarkable characters of remittent fever in its most unquestioned form. And if this particular mode of termination or evanescence never occurs in the fevers of contagion, or in the true typhus, which, from all my reading and observation is the fact, then is it at least proved, or rendered highly probable, that these minor fevers do not belong to typhus, however physicians may still determine to persist in referring them to the several doubtful or imaginary causes formerly discussed."

The great remedy, in all cases of obstinacy or relapse, is change of air, of habits, and of scene. Hence travelling, which combines all these, is equal to them all. In respect to medicine, the catalogue is small. The bowels should be regulated by the mildest means—the diet should be nutritious and plain—and light bitter tonics should be exhibited to improve the digestive process. These means would do more than all the farrago of drugs in Apothecaries' Hall.

We are so convinced of the fact, that a great number of disorders of the general health result from bad air, that we have taken great pains to concentrate Dr. Macculloch's views, and diffuse them widely through the profession. They are calculated to do much good, though some of them may have been carried too far. In succeeding articles we will take up the other subjects treated of in these volumes, and hope to render them still more interesting than the present—which, however, forms an indispensable preliminary to the investigation of malarious diseases generally.

From the Medico-Chirurgical Review.

ON FUNCTIONAL AFFECTIONS OF THE HEART AND ARTERIES. By M. Laennec.

In our last number we noticed the subject of neuralgia of the heart, and also angina pectoris. We shall now advert to some other nervous affections of this organ.

Palpitation.—Every beating of the heart which is sensible and unpleasant to the individual, and, at the same time, more frequent than natural, is termed palpitation. The pulsation is often audible by the patient, and even by the by-stander. If a person lies in a horizontal posture, during palpitation of the heart, he will hear, in that ear which is next the pillow, a pulsation double in number to that at the wrist. This arises from his hearing the alternate contractions of the auricles and ventricles. In many cases, there is only an increase of frequency in the actions of the heart, while the patient imagines, from his sensations, that there is also great increase of force. Laennec has known this kind of palpitation go on for eight days, the pulse remaining extremely small and weak, and from 160 to 180 in the minute. In healthy persons, and from the excitement of moral or physical causes, there will be an increase both of force and frequency in the heart's action. As the sound and sphere of the heart's pulsations are much increased during palpitation, we should never draw any conclusions from auscultation at such times. We must wait till the heart is in its usual rate of going.

But to speak of purely nervous palpitation, unaccompanied by any organic change in the heart, it may be observed that this kind is often much more troublesome and distressing to the individual than that which is dangerous from its cause. Far from being removed by the most complete repose, it is, in general, most distressing during the early part of the night. It often prevents sleep—and it is often removed, or at least mitigated, by moderate exercise. There is no complaint which is more liable to lead medical men, especially young practitioners, astray, than palpitation of the heart. The following passage from Laennec is in perfect accordance with our own observations.

"The purely nervous palpitations consist in an increase of the impulse, sound, and particularly of the frequency of the heart's pulsations. A feeling of internal agitation, particularly in the head and abdomen, always accompanies them; also a limpid watery condition of the urine. The duration of palpitations of this kind is very variable: they may be momentarily excited by mental emotion; while, at other times, they seem to originate without any obvious cause, and continue for several years, especially in young persons who are at the same time both nervous and plethoric.—It is commonly imagined that such an habitual over-action of the heart as such palpitations imply, must at length give rise to hypertrophy of this organ. This is possible; but I must say that I have never seen any proof of the accuracy of this opinion. On the other hand, I am acquainted with individuals who have been habitually subject to affections of this kind, and who nevertheless exhibited no positive sign, either of hypertrophy or dilatation."

The physical signs (as revealed by examination with the ear) which distinguish nervous palpitation from that dependent on organic disease, are thus characterized by Laennec.

"In nervous palpitation, the first impression conveyed by the stethoscope is that the heart

is not enlarged. The sound, though clear, is not heard loudly over a great extent of chest; and the impulse, although appearing considerable at first, is really not great, as it never sensibly elevates the head of the observer. This last sign seems to me the most important and certain of any, when taken in conjunction with the frequency of the pulsations. These are always quicker than natural,—being, most frequently, from eighty-four to ninety-six in the minute. Nervous palpitations are rarely accompanied by any sign of determination of blood to the head or chest, except in old persons."

Palpitation of the heart is often increased by the very means which are taken to subdue it. Dr. Parry made it fashionable to attribute all nervous diseases to increased fulness or impetus of the blood-vessels, and therefore, the lancet was freely used in palpitations. There can be no doubt that there are many sedentary females who eat a great deal too much, and walk a great deal too little, and who, consequently, have a plenitude of the vascular system, and a preternatural mobility of nerve. These, when affected with palpitation, are benefited by blood-letting, abstinence, and even purgation. But, in most other subjects, and especially in hypochondriacal and hysterical individuals, the depletory practice increases the palpitation. The complaint is, in fact, extremely indomitable, and it is a great object to be able to say that it *is* nervous palpitation, and unaccompanied by danger. When a patient is thoroughly satisfied on this point, he or she will be less anxious about a cure, and more easily reconciled to the presence of an uncomfortable companion. There are, however, some means which occasionally relieve or remove this complaint. As this affection seldom exists, without some cause which can be traced, by accurate investigation, to errors in regimen, mental distress, or derangement of some corporeal function, so this inquiry should never be omitted. At all events, there can be no safer or more effectual plan of treatment than that which is based on temperance, regularity, and the improvement of any deranged function. Failing in these efforts, we may prescribe a steady system of exercise—the shower-bath—and certain *sedatives*, in which we have so great faith. The mobility of the nervous system is best reduced by that which gives natural tone and strength to the whole system. Plain food, regular exercise, and early hours, will do more than all the assafœtida, bark, and valerian, in Apothecaries' Hall.

Irregularities of Action.—These may exist without palpitation. Irregularity of this kind is often met with in elderly people, without any disturbance of the general health. Sometimes, amid a series of pulsations, very unequal among themselves, a single one will occur one half shorter than the rest. This produces something like an intermission—and it completely resembles the latter if the pulsation be very weak. "These irregularities, as to frequency, take place most usually in persons affected with dilatation of the heart." The variations under consideration occur only in the heart, as heard by the ear or stethoscope —they make no sensible impression on the pulse, as felt at the wrist. We shall, therefore, pass on to more palpable irregularities of the heart's action.

Intermissions of Pulsation.—By this we understand, of course, a sudden and momentary suspension of the pulse, during which the artery is no longer perceptible beneath the finger. The duration of this intermission is very variable—being sometimes longer, sometimes shorter, than a common pulsation. There are two kinds of intermission—one *real*, consisting in an actual suspension of the heart's contraction—the other *false*, resulting from contraction so feeble as to be incapable of perception by the finger on the artery. Intermissions of the first kind are most common—they are frequent in old age, even when the health is good—or when the indisposition is only slight. "In middle age, they are *only* observed in certain diseased states of the heart, particularly hypertrophy of the ventricles, and during palpitation." In this, we cannot agree with Laennec. We have seen many instances, "in middle age," where temporary intermissions of the pulse were occasioned by flatulence, indigestion, acidity in the stomach—nay, even by emptiness of that organ,—that we rarely attribute any importance to this phenomenon, unless it be accompanied by other symptoms indicative of disease in the central organ of the circulation. Our author informs us that, by means of the stethoscope, we can clearly ascertain that "this species of intermission always succeeds the contraction of the auricles —it therefore differs only from the natural quiescence after this contraction in the irregularities of its recurrence." "If," says he, "in our examinations, we content ourselves with feeling the pulse, without applying the stethoscope, we shall, of necessity, confound this true intermission with the false one formerly mentioned produced by variations in the duration and force of the heart's pulsations."

"The last species of intermission is that which consists in the absence of one complete pulsation, recurring sometimes with an exact periodicity, after longer or shorter intervals, the pulse being in other respects regular. This pulse constitutes, according to Solano, the precursor of a critical diarrhœa. This peculiarity of the circulation is by no means rare; I have observed it frequently in some epidemics, but not at all in others, owing no doubt to the particular constitution that prevailed. This kind of intermission corresponds more frequently to a contraction of the ventricles, much weaker than the rest, than to a real interruption of their action; and, indeed, in such cases we often perceive an extremely feeble pulsation in place of a total intermission."

In a note appended to this passage, Dr. Forbes states as follows:

"In certain cases of diseased heart I have observed this species of intermission under a form which was sometimes productive of cu-

rious results. Every second pulsation was so feeble as to be altogether or almost entirely imperceptible. In the former case, the pulse appeared to be quite regular and slow; but, while in the act of feeling it, the intermediate or *latent* pulsation (if I may use the expression) became suddenly distinct, and the pulse was instantly *doubled*. In this manner I have known the same patient with a regular pulse at fifty or sixty, and a regular pulse at one hundred or one hundred and twenty, within the space of three minutes."--*Transl.*

Dr. Johnson was the first to point out this species of intermission several years ago, in the case of a gentleman residing in Bond-street, a patient under the care of Mr. Cosgreave of this metropolis. In this case, the ventricular actions were usually double those of the tangible arteries. But when any feverish excitement took place, the pulse became double the usual number or more, at the wrist, and corresponded exactly with the pulsations of the heart against the ribs. In by far the greater number of cases where we have observed intermissions of the pulse, there was an action—evidently a ventricular action of the heart, at the moment of the intermission at the wrist. Laennec has filled some pages with very obscure, not to say unintelligible reasoning respecting certain peculiarities of the pulse, without attempting any explanation of the cause of intermissions. One of the conclusions to which this great pathologist has come, is, in our opinion, a great error—namely, an independent pulsation in the arteries, without any impulse from the heart.

From an attentive observation of this phenomenon (intermission of the pulse) and where we have had very good opportunities of investigation, we have come to the conclusion that, in all cases, it depends on an unsuccessful action or contraction of the ventricle—not on an intermission of the ventricular contraction. The causes, however, of this abortive action of the ventricle are various. In very many cases, it is dependent on sympathetic associations of the heart with other organs, especially with the abdominal viscera; in which case, the intermission of the pulse is not constant, but only temporary. Where there is a permanent irregularity in the action of the heart or in the pulse, we believe there is generally some valvular disease, or alteration of structure. It may admit of much doubt, indeed, whether disease of the semilunar valves, or of the mitral valve, be most productive of intermissions of the pulse. For our own parts, we are inclined to impute permanent intermissions more to imperfections in the former than in the latter apparatus. It is curious that neither Laennec nor his translator has alluded (as far as we can see) to valvular disease, as the cause of intermissions and other irregularities (we always mean *permanent* irregularities) of the pulse. Yet we think these irregularities, when not merely temporary nervous affections, are, in three cases out of four, dependent on this cause.

Spasm of the Heart—with Bellows-sound and Purring-thrill.--Although the sounds above mentioned frequently attend organic diseases of the heart, yet it is certain that they may exist in consequence of a purely nervous affection. But in these cases, "it is always attended by symptoms which constitute a real state of disease." The bellows-sound is most commonly heard in hypochondriacs—particularly in those of a sanguine and plethoric temperament—in which cases the sound is usually heard in some of the arteries at the same time—frequently passing from one to the other. It is sometimes continuous—sometimes intermittent:--in the latter case, it recurs on the slightest agitation of the body or mind. The symptoms which accompany it are the more severe in proportion as the sound is greater, more continuous, and extending to a greater number of arteries.

"When it is very constant and distinct, but confined to the heart, there is almost always more or less dyspnœa, with a feeling of greater or less debility, so that the patient can, in many cases, hardly walk. These symptoms are still more marked, if the purring-thrill accompanies the bellows-sound. There is commonly but slight nervous agitation, particularly when the patient is quiet; but on attempting to walk rather quick, or for any length of time, he is soon out of breath, and, in the severer cases, the head becomes confused."

When this affection is unconnected with any organic lesion of the heart, it must be treated as a nervous complaint.

Neuralgia of the Arteries.—Pains more or less acute, continued, or intermittent, sometimes follow the course of the arteries, and appear to have their seat in the nervous filaments supplied to these vessels by the ganglionic centre. They occur most frequently in hypochondriacs and in nervous females. A blister to the part thus affected, is considered by Laennec the most effectual application.

Preternatural Pulsations of Arteries.—These are regarded by Laennec as convincing proofs "that the arteries have an action of their own, independent of that of the heart." When any one will show us a pulsation in an artery, when there is no corresponding ventricular contraction in the heart, then, and not till then, will we believe that the arteries can pulsate independently of any impulse from the heart. Such a phenomenon we have never yet seen—and we verily believe that such a phenomenon has never been seen or felt by man. But Laennec says that one carotid will be found to beat more strongly than another—and this is another proof of the dependence in question. We do not think so. If the arteries have a *pulse* independent of the heart, that pulse must consist in the *distention* of the vessel—for its contraction surely could not be felt by the finger. And is Laennec or any other man prepared to say that the arteries have a power of *self-distention* sufficient to constitute a pulse, or throb, independent of the rush of blood sent from the heart? If one artery is found to beat more strongly than another, are we sure that there is nothing which impedes the flow of

blood *into* the latter, or *out of* the former? We apprehend that the inequalities of pulsation in arteries are more dependent on such circumstances than on any power which an artery may possess of more strongly dilating at one time or place than at another. Laennec instances the increased pulsation of an artery leading to an inflamed part, as to a whitlow. Is not this a case directly in point, and corroborating our position?

From the London Medical and Surgical Journal.

a. *Observations on the Nature and Treatment of Erysipelas, illustrated by Cases.* By W. Lawrence, Esq., F.R.S. &c. Surgeon to St. Bartholomew's Hospital.*

b. *On the Treatment of Erysipelas, by numerous Punctures in the affected part.* By R. Dobson, M.D., Surgeon to the Royal Hospital, Greenwich.*

c. *Case of Erysipelas, with some Remarks.* By A. Copland Hutchison, Esq., F.R.S. L. and E. Surgeon to the Lord High Admiral, &c.*

There is, perhaps, no disease in the whole catalogue of human maladies deserving more attention than erysipelas. The frequency of its occurrence, and the discrepancy of opinion among medical practitioners respecting its nature and treatment, render it an important subject of inquiry. It is well known that the treatment of this disease by different practitioners has varied according to their views of its pathological characters, and that it has been conducted upon principles totally opposite to each other. This is not much to be wondered at, when we consider the various aspects presented by the affection in different subjects; that its characters vary according to the constitution of the patient; according to his local situation; according to the exciting cause, and the seat of the malady; and according to numerous other causes under whose influence the patient may be placed. This circumstance has led some to regard erysipelas as a disease depending upon a debilitated state of the system, requiring tonics and stimulants for its removal; whilst others have considered it as an affection differing in no material respects from phlegmonous inflammation. The latter view leads, of course, according to the modern pathology, to a plan of treatment consisting of blood-letting, and other antiphlogistic agents.

Now, looking impartially upon these opposite views, and taking into consideration the principal facts connected with erysipelas in its various aspects under the influence of different causes, we cannot help regarding it as a disease, if not quite unconnected with, at any rate not essentially dependent on, either a plethoric or a debilitated state of the system. Without travelling further in search of proof to support this opinion, the circumstance of the disease occurring in every grade of constitution, as regards plethora and debility, is amply sufficient. Did it require further proof, the fact that the malady is curable, in some instances, by the antiphlogistic treatment, and in others, by the stimulating and tonic plan, might be adduced. Plethora and debility are terms frequently used with the view of explaining particular conditions of the system which render it susceptible to particular diseases, but these terms have never, so far as we are aware, been defined in a manner which would render their meaning unequivocal. Indeed, if the word plethora mean vascular fulness, and a preponderance of nutrition over interstitial absorption, we frequently find it combined with very great debility; whilst, on the other hand, a condition of the vascular system opposite to fulness, and the absence of interstitial fat, are found compatible with perfect health and strength. Every practitioner must have witnessed instances where stout, and apparently strong and plethoric, patients could ill bear the loss of blood, or much purging; subjects habituated to fermented and spirituous liquors often present physical characters of this description; but it is well known that such constitutions, though plethoric in appearance, and even in fact, so far as regards vascular fulness, are characterized often by debility, and that their diseases, although generally attended by inflammatory symptoms, still require tonics and stimulants for their treatment. We find, on the contrary, that many persons, who, from outward appearances, might be supposed to possess but little strength, and no more blood in their vessels than barely sufficient to carry on the functions of life, nevertheless bear the antiphlogistic plan of treatment well, even to the abstraction of a considerable quantity of blood.

These are facts which ought not to be disregarded in laying down principles of pathology and of therapeutics; otherwise they will stare to shame every theory, however ingenious, which may have the indiscreetness to neglect them. It has always appeared to us, that the *quality* of the blood has much more to do than its *quantity* in predisposing the system to disease. We are well aware that this view is contrary to the pathology of the present day, which attributes every disease to some change in the mechanical condition of the solids. We shall offer a few further remarks on this subject as we proceed.

Mr. Lawrence regards erysipelas as an affection essentially inflammatory, and he considers the notion, that the local seat of the disease, the constitution, or both, are in a state of debility, to be "completely erroneous, and the treatment founded on it, not only inappropriate, but injurious." By erysipelas, he understands "inflammation of the skin, either alone, or in conjunction with that of the subjacent adipose and cellular tissue." When the surface of the skin is alone attacked, without any sensible swelling or vesication, the affection is called *Erythema.* "*Simple erysipelas* is a more violent cutaneous inflammation, attended with effusion into the cellular substance, and, generally, with vesication.

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* Medico-Chirurgical Transactions, Vol. xiv.

Phlegmonous erysipelas is the highest degree of the affection, involving the cellular and adipose membrane as well as the skin, and causing suppuration and mortification of the former." So far as the local disease is concerned, we are not aware that its inflammatory nature has ever been doubted. In fact, the seat of the affection presents all the characters of inflammation, namely, pain, redness, increased heat, and swelling. The question respecting which pathologists have differed, and do still differ, in opinion, is, not whether there be inflammation in a part affected with erysipelas, but whether that inflammation be as local in its character as inflammation purely phlegmonous, and whether the treatment should be exactly the same as in phlegmon? If the inflammation be the same in both, why applying different terms to it? Why, sometimes, calling it phlegmon, and at other times erysipelas? In fact, the term inflammation conveys but a very imperfect idea of the nature of disease, and our constitutional treatment of some inflammations would be worse than useless if we allowed ourselves to be guided by the local appearances alone. A prick of the finger is followed by inflammation; the same effect follows the insertion of a virus under the cuticle; it follows the application of heat; the application of cold will also produce the same effect, if the part be afterwards exposed to a moderate temperature, a temperature naturally congenial to the feelings; inflammation takes place in different parts of the body spontaneously, or from some internal causes, of whose mode of acting we are ignorant. All these causes are capable of producing diseases presenting the same local characters; at any rate, presenting the characters of inflammation. But let us examine the next grade in the order of effects: the inflammation caused by a prick of the finger will generally end, in a few days, in resolution, or, if not, it will form an abscess, containing pus: that caused by virus will be succeeded by a diversity of effects, according to the nature of that virus; for instance, the vaccine virus will form a colourless vesicle; the syphilitic virus will produce a progressive destruction of the part to which it is applied; that of small-pox is followed by effects different from those of either of the former, although the first effect of the application of each to an absorbing surface is inflammation. Inflammation caused by the application of heat is succeeded by a detachment of the cuticle from the subjacent skin; whereas, that produced by cold runs suddenly on to mortification, seldom terminating in the formation of an abscess. With regard to the treatment found best adapted to these different affections, it varies nearly as much as the appearances themselves. In fact, if we were to regard the inflammation as the sole disease, the same treatment ought to be found to answer in every inflammation. Experience, however, proves that this will not succeed. We have already advanced the opinion that bleeding would, probably, be beneficial in the majority of diseases, especially when the system at large is affected, if it could be performed without detriment to the functions of the organs generally. Upon the same principle, it would modify every variety of inflammation; but it will also modify diseases unattended by any inflammation, such as those allied with the nervous system. To suppose, however, that general bleeding will suffice to cure every variety of inflammation, or that it can be always resorted to in inflammatory diseases without increasing the derangement of the general functions, or even the functions of the seat of the inflammation, would be supposing a thing contrary to the testimony of experience. We can only regard inflammation as a system of a variety of diseases, differing materially in all their other characters, and requiring various modifications of treatment. Small-pox, measles, scarlatina, psora, and many other affections, produce inflammation of the skin; but the other characters of these maladies differ as much from those of each other, as they do from the characters of erysipelas, or of phlegmon. Indeed, we find precisely analogous differences between the characters of phlegmon and those of erysipelas, whether we look at the constitutional or at the local symptoms.

We have already stated that Mr. Lawrence divides erysipelas into *simple* and *phlegmonous*. "In simple erysipelas, the skin is preternaturally red and shining, having a light rosy tint in the early stage and slighter cases of the affection, while in other instances it is of a bright scarlet, or even of a deep and livid red." In this slight form of the disease there is hardly any perceptible swelling, and no tension. The affection, however, is seldom confined to the skin alone, for effusion soon takes place into the cellular texture, giving rise to a soft swelling. The inflamed part is hot, painful, and imparts a sort of smarting or stinging sensation. The pain is not so intense as in phlegmon, nor has it the same throbbing character. Serous effusion takes place from the surface of the cutis, elevating the cuticle into vesicles or blisters, or "raising it by a soft, yellow, jelly-like deposit, which remains slightly adherent to both cutis and cuticle, and exactly resembles the effect often produced by the common blistering plaster." The inflammation, so long as it is confined to the skin, does not produce suppuration. It may do so, however, when it becomes very severe at one particular point, "and we thus occasionally see the formation of abscess under the skin, towards the decline, or after the disappearance of the erysipelatous redness."

Mr. Lawrence further remarks, that "the local symptoms above described are preceded and accompanied by fever, which varies in its character according to the constitution, age, and general state of health." This fever has an inflammatory character in the young, strong, and those of full habit, and blood taken from a vein exhibits the inflammatory crust on its surface. The fever, in other instances, is of the typhoid type, particularly when ery-

sipelas attacks the head. The author thinks that the pain felt in the epigastric region, foul tongue, with bad taste in the mouth, nausea and constipation, indicate "disordered stomach and intestinal canal, to which, as its *cause*, the local affection must be referred." We grant that these symptoms are indicative of disorder of the stomach and intestines, but Mr. Lawrence should have given us some proof that the local affection is referrible to this disorder, as its *cause*. The head suffers pain as well as the stomach; what proof is there, then, that the local malady does not depend as much upon disorder of the brain as upon that of the stomach and intestines? Again, the heart is excessively irritable: if the disease of one seat must be referred to that of another, why is not the inflammation of erysipelas not as referrible to a disorder of the heart, as to gastric derangement? Headach and increased irritability of the heart both precede and accompany the local inflammation, as much as nausea, foul tongue, and constipation do. It appears to us, that there is no more reason to attribute the local disease to disorder of the stomach and intestines, than for attributing the disorder of these organs to the local disease. The stomach and intestines, as well as the brain and heart, bear a part in the general disorder, and every organ manifests a derangement of its function according to the nature of its office. The stomach proves itself deranged by nausea; the intestines, by constipation; the brain, by headach and delirium; the heart, by increased irritability and quickness of motion, with a reduction of its absolute strength; the nervous and muscular systems, by lassitude and a general feeling of weakness. In fine, every organ, and even every tissue, as far as its functions are cognizable by the senses, or can be inferred from external signs, exhibits derangement; and if the local affection were referrible, or were attempted to be referred, to a disordered state of any other seat than that alone in which it resides, we should find it rather a difficult matter to point out any one organ which could lay a greater claim than the rest to that local disease. There is often not even a priority of affection to be discovered on the part of the stomach and intestines; for headach, quickness and irritability of the pulse, as well as many other symptoms of disorder, present themselves to our notice quite as early as gastric derangement. But a priority of affection, could it even be proved, would constitute no proof of dependence of the local malady upon gastric disorder. Erysipelas often follows as an affect of injury: what reason is there here to refer it to disorder of the stomach? or to any one organ more than another?

The inflammation in phlegmonous erysipelas is more deeply seated, and in a higher degree, than in the simple form of the disease. It occupies the whole thickness of the skin and the subjacent adipose and cellular tissue, and it soon runs on in the latter tissue to suppuration and sloughing. The general fever also is more violent than in simple erysipelas. The nervous system is often considerably affected, and the symptoms occasionally assume the character of those indicative of the worst form of typhus fever. The local inflammation is of a dark red colour, often of a brownish or livid tint; effusion of serum takes place into the cellular membrane, producing considerable tumefaction. This yields under the pressure of the finger, and occasionally retains the mark of the impression for some time. At first the cellular texture contains a whey-like or whitish serum. The fluid gradually becomes yellow and purulent, and we often find it presenting all the characters of good pus, and very thick. The matter is sometimes deposited in small separate collections, forming little abscesses; but it oftener fills a large portion of the cellular membrane, without having any distinct boundary. This tissue frequently sloughs to a considerable extent. The skin also, thus losing its supply of blood, dies, sometimes all round a limb, and to a very great extent. When this is the case, the constitutional disturbance is, of course, very great, and the patient generally sinks under it.

Mr. Lawrence next speaks of the seat and nature of erysipelas. With respect to the seat of the disease, he considers it to be the skin and cellular tissue, and not the aponeurosis of muscles, as Mr. C. Hutchison thinks, or the subcutaneous tissue and fascia, as Mr. Earl believes. In this opinion we fully agree with Mr. Lawrence, that the skin and cellular membrane are the tissues which are primarily affected; but we have, nevertheless, reason to believe that the fascia often becomes involved in the disease in phlegmonous erysipelas. We much doubt that the aponeuroses of muscles are ever primarily or principally affected. These are of a texture similar to that of a tendon, and we have frequently noticed tendons, perfectly unaffected, exposed in, or traversing, chasms produced by the sloughing of the cellular tissue, and appearing as white and clean as if they had been dissected for demonstration. So far, then, we agree with Mr. Lawrence, that "erysipelas is merely a particular modification of cutaneous or cutaneous and cellular inflammation."

But Mr. Lawrence's attempt at comparison between erysipelas and phlegmon appears to us quite contradictory. In one paragraph we are told, that "the difference between erysipelas and phlegmon is not merely in the original seat or degree of the disturbance; there is also a difference in *kind*." Of this difference we entertain no doubt. But, in the very next paragraph, the author says that he "can, however, by no means agree with those who regard it (erysipelas) as a distinct *species* of inflammation, and as capable, in that character, of affecting various parts of the body as well as the skin." Surely, if erysipelas differ in *kind* from phlegmon, and from every other inflammation, it must likewise differ in *species* from every other. As we are by no means fond of hypercriticism, we shall not

quibble about the meaning of a word or two. When Mr. Lawrence says that erysipelas is not to be regarded as a distinct species of inflammation, he means that the term erysipelas, ought not to be applied to "certain inflammations of the conjunctiva, mouth, and fauces; of the respiratory and alimentary mucous surfaces; of the serous membranes in the head, chest and abdomen, and of the brain, abdominal and thoracic viscera." He says that the proof of the identity of these various inflammations would consist in showing, that the same peculiarities which distinguish erysipelas from other inflammations of the skin are found in certain inflammations of the parts just enumerated.

Now, we consider Mr. Lawrence's view in this respect erroneous, and it is probable, if he were to reconsider the subject, he would discover this error. He remarks, that "since the distinguishing characters of erysipelas are clearly referrible to the peculiarities of the cutaneous and cellular structures in which it occurs, we could not expect to meet with the same affection in parts so differently organized as serous membranes and the viscera." We may, in the first place, show, that the mucous and serous membranes do *not* consist of tissues so very differently organized from the skin and cellular membrane as to lead us to suppose, *à priori*, that they could not be subject to the same diseases. Mr. Lawrence himself admits that both the skin and cellular membranes are subject to erysipelatous inflammation. Now, there is a much greater difference between the organization of the skin and that of the cellular tissue, than between the skin and the mucous membranes. The latter are very nearly allied, and are, in fact, continuous with one another. With the exception of the cuticle, the membrane lining the mouth and fauces is pretty nearly the same, in organization, as that covering the outside of the lip and face. The former may, truly, be of rather a more delicate texture than the latter; but we find that the skin covering the inside of the thigh is much more delicate, and thinner, than that which coats the outside of the same limb. Again, let us compare the cellular, with the serous, membranes. The former of the two has the property of upholding two characters, and one of these identifies it with the *serous* membranes. One surface of the membrane is serous, and the other cellular, destined for the secretion of fat. The physiological characters, then, of this tissue are not so very different from those of the serous membranes generally as might be at first supposed. Again, some of the pathological characters of the two membranes are very nearly allied. We find dropsy of the cellular membrane accompanied with dropsy of the peritoneum, pleura, pericardium, and the serous membranes of the brain. These facts, therefore, render it highly probable that the difference in organization between the serous and cellular tissues, is not so great as to lead us to suppose the former not to be subject to erysipelatous inflammation.

Moreover, admitting, as Mr. Lawrence maintains, that the skin and cellular membrane are the only tissues subject to erysipelas, as the latter pervades almost every part of the system, interweaving the other tissues, and as it is similarly modified in every seat, Mr. Lawrence should show us some reason why it should not be as subject to erysipelas in one seat as in another. If that portion of it which unites the skin to the subjacent fascia be a principal seat of the disease, why should not other portions of the same membrane, intervening other tissues, such as the coats of the intestines, the pleura and lungs, the coats of the œsophagus, &c., be also liable to be attacked, if erysipelas be guided in its choice of seat, as we believe it is, in some measure, by the modification of the organization? He should also point out what these peculiarities are, "which distinguish erysipelas from other inflammations of the skin," and which are not to be found in certain inflammations of other parts. The only peculiarity we know of in this respect is vesication. But let us ask, how can vesication take place in parts which have no cuticle to form vesicles? In other respects we can perceive no material difference between the characters of cutaneous erysipelas, and, what we should call, erysipelas of internal parts. Erysipelas of the face is often accompanied by inflammation of the fauces, which the author seemes to admit; but he, at the same time, states, that this inflammation of the fauces has "only one character in common with erysipelas, namely, redness. The swelling and vesications of erysipelas are not found in these inflammations, which, on the other hand, are frequently attended with *ulceration*, with the formation of an ash-coloured or tawny substance adhering to the surface, and with superficial sloughing."

Now, it is evident that no vesications can take place in the fauces, because the skin there has no cuticle; and, this being the case, is not *ulceration* the very effect that we ought to expect to occur in a part organized as this is, as a substitute for vesication? The sloughing, also, which occasionally takes place in the fauces, is perfectly in character with erysipelas. We are told that no *swelling* attends these internal inflammations. Now, Mr. Lawrence himself tells us that, in simple erysipelas, "if the skin alone be affected, *there is hardly any perceptible swelling*, and no tension." We could not, therefore, expect to find swelling in simple erysipelas of the fauces, or of any other superficial membrane. The swelling and tension are produced by effusion of fluid underneath the covering membrane, in the cellular tissue; so that the swelling cannot be very great in parts where there is a scantiness of the latter membrane, and especially if the inflammation be superficial. Upon the whole, then, we consider that Mr Lawrence has completely failed to establish his point—that erysipelas is confined to the integuments and the subjacent cellular tissue alone. In fine, we may adduce the fact, that the mucous membrane of the

nostrils often becomes involved in the disease in erysipelas of the face. We perfectly agree with the author, that the term erysipelas ought not to be confined to inflammation of the skin alone, to the exclusion of that of the cellular membrane, as has been proposed by Mr. Earle and Mr. Arnott. There can be no difference here in the essential nature of the disease, inasmuch as both tissues are simultaneously involved in the affection. But when Mr. Lawrence endeavours to identify erysipelas with phlegmon, he appears to us to fail to establish his point. Of the essential nature of disease we know no more than what may be inferred from its phenomena, and from the causes which appear to give rise to it. But from every fact connected with phlegmon and erysipelas, we have ample reason to consider them essentially different. It is true that the inflammation caused by external injury may assume the characters of either phlegmon or erysipelas, according to the constitution of the patient, or to certain external causes, under whose influence he may be circumstanced; but we never find inflammation of a phlegmonous character propagating itself by contagion, or prevailing epidemically. We need only refer to the first number of this Journal, wherein a review is given of Dr. Gibson's essay on the epidemic erysipelas which prevailed at Montrose, in 1822, for proof of the contagious nature of this malady, and of the identity of erysipelas with certain inflammations of internal parts.

Mr. Lawrence says that he is quite at a loss to discover in erysipelas those marks of debility which some have so much insisted on. We said before, that we are by no means fond of hypercriticism when the subject of inquiry is of trifling import; but, as erysipelas is a disease deserving minute attention, we must be allowed to lay more stress upon, and be more particular about, certain terms than some might consider necessary in the analysis of a work of this description. Having thus apologized, we may be permitted to ask for a definition of the word *weakness*, according to the sense in which the author means here to apply it. We are not aware that any disease depends essentially upon *strength*, according to the common acceptation of the word. Strength and weakness are merely relative terms, and have, perhaps, less to do with the production of disease than is generally supposed. The strength of a man of thirty, comparatively very weak for his age, would be considered very great if possessed by a child of eight or nine years old. Again, a person may possess great muscular power with a weak heart, or, *vice versa*, he may possess a strong heart with little nervous energy. If a person be predisposed to disease, he is necessarily weaker than he would be, *cæteris paribus*, if he were not so predisposed; therefore, every person, when attacked with erysipelas, or with any epidemic disease, or any casual malady not caused by violence or injury, may be said to be in a state of weakness at the time, because a certain degree of predisposition, most probably, always precedes the full development of such maladies. Again, vascular fulness and muscular weakness are perfectly compatible with each other, and we find them associated every day in the same constitution. But, if we suppose the term weakness to relate to the standard of muscular strength and nervous energy possessed by the majority of mankind at a given age, we shall find that erysipelas selects its victims more frequently from among persons who rank below that standard, than from amongst those who rise above it. We, however, by no means attribute the disease to the weakness itself, but to another cause, of which the weakness, as well as the malady, is only the effect. Weakness, according to the general acceptation of the word, is itself an indication of a state of constitution at variance with perfect health, although it may not always be accompanied by disease in its full development, or by disease characterized by sensible and unequivocal external signs.

Mr. Lawrence remarks, that "however weak the patient, the local disturbance is one of excitement; there is increased activity in the circulation of the part clearly marked by all the symptoms. Indeed, speaking of the part," he is, "unable to recognise debility as the cause of any inflammation whatever; and in reference to the seat of disease," he regards "the expressions of passive and asthenic inflammation, and venous congestion, as either unmeaning, or calculated to convey erroneous notions."

Now, from the above passage, we can pretty clearly understand what Mr. Lawrence's views of the pathology of inflammation are: they are by no means peculiar; but any one, who will take the trouble to examine the condition of a part in a state of inflammation, may satisfy himself that they are erroneous. Instead of there being "increased activity in the circulation of the part," if that part be examined with the microscope it will be seen that the activity is much less than when it is in a healthy state. The motion of the blood is much slower in it than natural, as might be inferred, *à priori*, from an acquaintance with the laws of hydraulics. The visible characters of inflammation depend upon a preternatural enlargement of the caliber of the arteries of the part affected, which enlargement permits the vessels to contain more than their proportionate share of blood; but owing to this disproportion in the size of their canals, the velocity of the fluid through them is necessarily less than if their caliber were natural. This is a fact, as we have already stated, of whose truth any one may satisfy himself by microscopical examination. Even supposing the arteries to be active agents in the circulation of the blood, we may ask, by what power, or mode of action, could a part acquire more than its due share of blood, according to the notion of there being increased activity in the circulation through it? The arteries have never yet been considered endued with a power of attraction. If, then, as

Mr. Lawrence must necessarily mean by increased activity, this activity is applied to the propulsion of the blood, an inflamed part, instead of containing a superabundance of blood, as it invariably does, ought to be paler than natural, and to contain less than its due share, because the increased activity would necessarily drive the contents of the vessels out of that part in which it resided. The only way in which the visible phenomena of inflammation, according to this view of the function of the arteries, could take place, would be, by the vessels *leading towards* the inflamed part assuming an increased activity, whilst those situated actually *in the seat of inflammation* remain in a state of comparative *inactivity*. Supposing this to be the case, though contrary to the fact, it would by no means prove that there is an increased activity *in* the inflamed part: on the contrary, it would only prove an increase in the activity of the vessels of a *contiguous* part, and a comparative decrease in that of the arteries of the actual seat of the disease.

Moreover, Mr. Lawrence acknowledges his inability of recognising debility as the cause of any inflammation whatever. Now, if debility mean a want or the absence of a power natural to a living part, we cannot understand how any inflammation can take place without debility. The only living power connected with the arteries, so far as we can recognise, is *contractility*. The arteries of an inflamed seat lose this property, in a great measure, and allow themselves to be preternaturally dilated by the pressure of the blood. Were it not for debility of their coats, or a reduction in the amount of contractile power natural to them, their caliber could not enlarge, as it does in vessels undergoing the process of inflammation. To this cause is to be attributed the redness of the inflamed part, as well as the swelling at the commencement of the attack, before effusion takes place into the interstices of the tissues. The pain, and the preternatural evolution of heat, in the seat of affection, are attributable to the organic derangement which takes place in the coats of the vessels, and which acts as the cause of their loss of tone or contractility. These latter phenomena, namely, the heat and pain, always precede the redness and swelling, inasmuch as they rank a grade higher in the order of causation. It is not requisite at present to trace causation further back than this derangement, because to do so would require to grasp at the original cause of disease, and would involve the fundamental principles of physiology and pathology.

Mr. Lawrence again says that, in reference to the seat of disease, he regards the expressions of passive and asthenic inflammation, and venous congestion, as either unmeaning, or calculated to convey erroneous notions. With respect to the terms active and passive, sthenic and asthenic, neither one nor all of them can convey any clear idea of the condition of an inflamed part, inasmuch as some of the functions concerned are more active than natural, whilst others are less active than in the healthy state. Evolution of caloric is augmented, whereas the contractility of the arteries is diminished. These are two properties essential to inflammation, upon the latter of which, the preternatural redness of colour depends. As for the other two, namely, pain and swelling, we can perceive no relation between them and the terms passive and active, or asthenic and sthenic. The nervous energy is exalted in the seat of disease, as far as regards sensibility; but it is diminished with respect to natural sensation, and also to the power of motion when the inflammation is seated in the muscular tissue. Secretion is generally increased in the part affected, but there is reason to infer that absorption is diminished. Hence, we not only agree with Mr. Lawrence, that the terms passive and asthenic, as applied to inflammation, are unmeaning; we go further, and say that the terms active and sthenic are equally unmeaning.

But, Mr. Lawrence draws "venous congestion" also within the sphere of these unmeaning terms. If by congestion be meant a preternatural collection of blood in the vessels of a particular part, can Mr. Lawrence, or any one else, deny that such a preternatural collection does take place in inflammation? What else is it that gives the part inflamed the unnaturally red colour which forms one of the most characteristic properties of every species of inflammation? But it is to *venous* congestion that he applies the epithet unmeaning. Now, we do not maintain that *venous* congestion is, by any means, essential to inflammation; but, that it is a frequent attendant on inflammation, any one may satisfy himself by examining the state of the cerebral veins when the meningeal membranes are inflamed; or let him examine the mesenteric veins in subjects who have died of peritoneal or intestinal inflammation.

We shall not offer any remarks on Mr. Lawrence's Nosological Arrangement of Erysipelas. With respect to the *causes* of the disease, he thinks that the occurrence of the malady in the face may be traced in some instances to contagion. Our opinion is, that its occurrence in other parts also may be traced to contagion, although various other causes may contribute to produce it. This has been the opinion of almost all the most eminent writers who have treated of this subject, and the history of the malady affords ample proof in support of the notion of its contagious properties. Mr. Lawrence says, that there is really no difference as to causes between erysipelas and other inflammations. We are fully aware that an injury inflicted on a part may be followed by either phlegmon or erysipelas, according to the constitution of the patient, or according to other circumstances under which he may be placed; but do we not find also that the same external causes will produce simple inflammatory fever in one individual, and typhus fever, of the most contagious nature, in another? Erysipelas may follow as an effect

of injury inflicted on a part, but the disease may become contagious from that time, owing to some peculiarity in the constitution of the patient. Several facts connected with the history of the disease prove this to be the case. Some diseases possess the property of propagating their kind, either by contact, or by the diffusion of a principle conveyed from one individual to another through the atmosphere. Now this principle must have some origin, either in the body or out of it. If a mechanical injury can create such a disturbance in a constitution peculiarly modified, as to give rise to the formation of this contagious principle in the system, the disease in that system, or in a local seat, must evidently assume the same characters as if the contagious effluvia, already generated, had been applied to the body; and the newly-formed principle may go on progressively from this time, and propagate its kind in constitutions which may be in any way susceptible to the poison, and which are brought within the sphere of its influence. The contagious principle, like other forms of existence, must depend upon the union of certain causes, each of which, separately, may be perfectly destitute of the property of generating disease. It differs little whether the principle of contagion be formed in the body, by the union of the causes necessary to its formation, or imparted, already formed, to the body, from another individual labouring under the disease.

We next come to the treatment of erysipelas. It is this part of Mr. Lawrence's essay which renders it highly interesting.

The treatment of simple erysipelas must be modified according to the constitution of the patient and the degree of the local inflammation. Mr. Lawrence recommends general blood-letting in the young and robust only, local abstraction of blood by cupping or leeching being usually sufficient in the generality of cases. In addition to local bleeding, the patient is to be placed upon the antiphlogistic regimen. In a word, the plan which the author recommends is purely antiphlogistic, the same as that pursued in inflammation in general. But he admits, at the same time, that the practitioner is occasionally obliged to have recourse to stimulants and tonics. He must be guided here, of course, chiefly by the character of the constitutional symptoms. Tonic remedies are by no means incompatible with local abstraction of blood. In the treatment of inflammation, there are two points to be particularly attended to: the first consists in allaying the irritability of the system and in endeavouring to subdue that constitutional disturbance which tends to wear out the energy of the nervous system: the second consists in subduing the local malady, before it shall have time to run on so far as to produce such a change in the organization of the part as totally to destroy its function, or even its vitality. Now, the important question is, how is the general disturbance to be best allayed? Without reverting at present to the *modus operandi* of bleeding, we find that in inflammation, if the patient can *afford* to lose blood from a vein, bleeding tends more than any other remedy to subdue the constitutional derangement. But there are limits beyond which blood cannot be safely abstracted, because a certain quantity of this fluid is necessary to the support of all the tissues and to the performance of their several functions. We know that the mass of blood is being constantly reduced by the several secretions and excretions, and that it can be but very slowly renovated when so little nourishment is taken into the system as is commonly the case when the general functions are so much disturbed. If then we abstract so much blood as to leave barely sufficient behind to carry on the functions of life, the mass will be further reduced by the causes already mentioned, and the organs will cease to perform their several offices. We are fully convinced that we have witnessed several cases of death produced by these causes. It is often a great object to take away as much blood as the system can well bear to lose; but if this be done, the energy of the constitution must be, at the same time, kept up by nourishing diet, and sometimes even by stimulants and tonics, otherwise it will sink for want of support. Sometimes the quantity of blood in the system, at the commencement of the attack, is no more than just enough to support the functions of the different organs, and would soon diminish so much as to be insufficient to do so, if not aided by stimulants and tonics. This state of the system may be accompanied by violent local inflammation, threatening to destroy the organization of the seat of affection. He who would resort to general blood-letting under such a circumstance, would soon find his patient slip through his hands.

But with respect to local blood-letting: an inflamed part of considerable extent deprives the general vascular system of a great proportion of blood. The caliber of the vessels in the seat of inflammation is much enlarged, so that the part constantly retains considerably more than its due share of blood. The support which this blood would impart to the system at large, if the fluid were equally distributed, according to the *natural* capacity of the vessels of the different seats, is now completely lost to it, whilst it is at the same time injurious to the part in which it resides. By bleeding locally, from the inflamed surface, we empty the vessels of that blood which is injurious to them, owing to its preternatural quantity, and, perhaps, owing also to its morbid condition; and we, by this means, afford their coats an opportunity of recovering their contractile property. We do this without reducing in any material degree the mass of blood circulating in the general system. Hence, as we before stated, local blood-letting, from the seat of inflammation, is perfectly compatible with the administration of nourishing diet, stimulants, or tonics, when these are found necessary to support the energy of the constitution.

If the extension of the local inflammation

cannot be arrested by the application of leeches and cupping, we have still a very powerful means in our possession, namely, "making incisions through the inflamed skin and the subjacent adipose and cellular textures, which are the seat of the disease." It is in this that the peculiarity of the plan of treatment pursued by Mr. Lawrence mainly consists. "These incisions are followed very quickly, and sometimes almost instantaneously, by relief, and cessation of the pain and tension; and this alleviation of the local suffering is accompanied by a corresponding interruption of the inflammation, whether it be in the stage of effusion, or in the more advanced period of suppuration and sloughing. The redness of the skin is visibly diminished during the flow of blood from the incisions; in twenty-four hours it has generally disappeared, and the skin itself is found wrinkled from the diminution of the general inflammatory tension." These incisions usually put a stop to the further extension of the local inflammation, and Mr. Lawrence says it has never failed to do so within his experience "when the case has been a proper one for the practice, and the state of the patient has admitted of its being fairly tried." There might be some difference of opinion respecting whether or not a case be a proper one for the practice. But no quibble can be raised upon this point, as Mr. Lawrence has given numerous cases to show which are proper and which are not.

Mr. Lawrence wishes to be understood, however, that he does not advise incisions in erysipelas generally. He confines their employment to cases of the phlegmonous kind. Much will, of course, rest upon the judgment of the practitioner, whether incisions be requisite or not. It is important to decide quickly upon this point, and to make the incisions without delay when they are considered necessary. When the disease "attacks the face, it is not attended with that serious inflammation of the subcutaneous structures which requires incisions." The author thinks, however, that they may be advantageously resorted to in the eyelids, when the inflammation is severe. This practice, although alluded to by some writers, several years back, is indebted for its recent introduction to Mr. Copland Hutchison. There is this difference between the manner of making the incisions, as recommended by these two surgeons, viz. Mr. Huchison recommends a number of incisions, proportionate to the extent of the inflammation, and about an inch or an inch and a half in length, through the skin and cellular tissue; whereas, Mr. Lawrence's plan consists in making *one* incision, extending from one boundary to the other, through the centre of the inflamed part. Dr. Dobson, again, recommends a great number of punctures to be made in the part affected, at a short distance from each other. These three modes of treatment appear to us to be founded upon precisely the same principle, namely, that of giving vent to the preternatural quantity of blood which distends the vessels in the seat of inflammation; and they appear to have proved equally successful from the account given of them by their several advocates. Each author, of course, thinks his own plan the best. This is very natural, if it has proved successful in his hands. Admitting that the three plans are equally efficient, the next question is, which is the most expedient. Looking at them impartially, it is our opinion that we ought to be guided mainly in this point by the situation and extent of the inflammation. When the face forms the seat of the inflammation, it is evidently of great importance to avoid incisions, which must necessarily leave scars behind, if punctures will answer the purpose as well. But when the disease is situated in a part generally covered by the clothes, we should prefer incisions, as they will give a freer vent to the congested blood.

When incisions are resorted to, it appears to us that their number ought to be determined by the *extent* of the inflammation. When it is of small extent, one incision, of some inches long, carried through the middle of the part, will sufficiently empty the vessels and relieve the limb of its tension. But sometimes the limb is inflamed all round, and to a very great extent. A single incision, extending from one end of a limb to the other, would present a terrible gash, and we doubt that it would afford the vessels the same facility of emptying themselves of their contents as a number of smaller incisions made in different parts of the inflamed surface would. By making a number of small incisions, the larger branches of the vessels may be avoided; for the efficacy of the practice cannot, evidently, depend as much upon the actual quantity of blood that may follow from the cut, as upon its being thrown out by the smaller vessels, whose coats have lost their contractility. If a large vessel be divided, the general system will be reduced by the loss of blood, without affording much relief to those which are preternaturally loaded. Indeed, the hemorrhage may be so profuse as to prove fatal.

Upon the whole, it appears to us that we ought not to allow ourselves to be prejudiced in favour of one of these plans more than the others, if they are found equally successful, and if the principle be the same in each, but to adopt that which may appear most applicable to particular cases. Whilst the local disease is thus treated, the constitutional derangement must also be attended to. The most valuable remedy in our possession for subduing inflammatory diseases, is mercury. This Mr. Lawrence highly recommends. Indeed, this will not interfere with the administration of other remedies, either antiphlogistic or tonic, as the case may require.

Mr. Lawrence, in the essay before us, relates thirty-eight cases of erysipelas. These are highly interesting; but having explained to our readers, his views of the nature and treatment of the disease, we do not deem it necessary to insert any of the cases.

From the Lancet.

ON THE CIRCULATION OF THE BLOOD IN THE FŒTUS,

*In Quarto, with Ten Lithographic Tables.**

This is a most interesting work; and we regret that, from want of space, we are obliged to give our readers only a very concise account of it. In the *first section*, a rapid view is taken of the different opinions on fœtal circulation; they may be reduced to the following three:—

1. The blood is conveyed by the venæ cavæ into the right auricle, passes through the foramen ovale into the left auricle, and from thence through the left ventricle into the aorta; the small portion which, from the right auricle, passes through the right ventricle and into the pulmonary artery is, by the duct. arteriosus, carried into the aorta. This is the opinion of Harvey, Fabricius ab Aquapendente, Morgagni, Merry, and Haller.

2. The blood of the vena cava superior is separated from that of the vena cava inferior, by means of the Eustachian valve, by which the blood of the cava inferior is directed through the foramen ovale into the left cavity, whence it passes into the aorta ascendens and its branches, the innominata, left carotid, and subclavian; the blood of the cava superior goes directly into the right ventricle, and from thence through the pulmonary artery and duct. arterios. into the aorta descendens. According to this opinion, which is that of Nichols, Sabatier, Bichat, and Bordeu, the head and upper extremities receive arterial blood, while the lower half of the body is supplied with venous blood. By this circumstance, Bichat endeavoured to account for the early development of the head, and superior extremities of the fœtus. As, however, the circulation must considerably differ according to the different conditions of the heart, and as this organ, during its formation, undergoes important changes in its form and mechanical arrangement, it appears that both opinions are defective and erroneous, because they overlook these metamorphoses.

Thus the different size of the cavities of the heart, in different periods, evidently contradicts the *opinion* of Sabatier, &c., that through the whole fœtal life all the blood of the cava inferior passes through the right auricle into that of the left side, and the blood of the cava superior goes directly into the right ventricle; for, *originally*, the left cavity of the heart is by far the largest of the two; at *a later period* they gradually become equal, and *shortly before birth* the right cavity surpasses that of the left side; whereas, the quantity of the blood of the cava inferior, exceeds, at *all periods*, that of the cava superior; so that, if the blood of the former did constantly go into the left cavity, this ought likewise constantly to surpass the other in capacity. It appears, consequently, that the opinion of Sabatier, &c., corresponds *only to the earliest periods* of embryonic life. Another very important objection is, the simultaneous contraction of both auricles, and their alternate motion, with that of the ventricles, so that neither the blood of the cava superior can go in an uninterrupted stream through the auricle into the left ventricle, nor the blood of the cava inferior be carried from one auricle into the other. Lastly, it is very *improbable*, that the head and upper extremities should be supplied with arterial blood, and the lower half of the body with venous blood, and the slower development of the lower extremities can by no means sufficiently account for such a difference.

With regard to the opinion of Harvey and Haller, the right auricle, especially in the earlier periods, is of too small a size to admit of all the blood of both venæ cavæ; at the same time the simultaneous contraction of both auricles prevents the passage of the blood from the one into the other. The use of the valvula foramen ovale cannot be to prevent the reflux of the blood into the right auricle, as during the simultaneous contraction of both auricles they are acting against each other, and thus mechanically preclude every passage but that into the ventricles. It appears that the structure of the *valvula foraminis ovalis*, and of the *Eustachian valve*, was not sufficiently known to the physiologists, and that the use of these organs was completely misunderstood.

We proceed to the *third opinion* on fœtal circulation: the vena cava has two openings, the one into the right, the other into the left auricle; the latter is the so called foramen ovale. By means of this arrangement, the blood of the cava inferior goes separately into each auricle, and no passage of the blood from one auricle into the other takes place. Originally, and up to the third month, the cava inferior opens *only* into the left auricle, and even, for a considerable time afterwards, this opening is by far *the larger* of the two; so much so, that the foramen ovale may be justly regarded as the principal opening of the vein into the heart, and that its subsequent exclusion is, in fact, nothing but the gradual obliteration of the opening of the cava inferior into the left auricle. The right auricle, and the Eustachian valve, must be considered as a continuation of the right side of the cava inferior; and the left auricle, with the valvula foramen ovale, as a continuation of its left side.

It is the principal object of our author to confirm and develop this opinion, which was originally established by C. F. Wolff, in the Acta Acad. Petropolit, 1777. It regards only the passage of the blood through the heart; as to its way through the aorta ascendens and descendens, Mr. Kilian adheres to the opinion of Sabatier, &c.

The *second section* contains a comparative view of Haller, Malpighi, Spallanzani, Pander,

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* H. Fr. Kilian, Ueber den Kreislauf des Blutes, &c.

and Wolff's investigations on the formation of the heart in the incubated egg. The observations of the author on the same process in the human fœtus, refer particularly to the original condition of this organ as a simple cavity, and of the aorta and pulmonary artery as one single canal, and the subsequent metamorphoses of these organs into complicated cavities and several vessels. According to this view, the course of organic formation strikingly corresponds with the peculiar type observable in the general gradation of organizations; a view which, by a comparison of the heart and the principal vessels in fishes, the different genera of amphibia, and in birds, with that of the human fœtus in its different periods, is found correct to a remarkable extent.

We cannot omit giving our readers a brief extract of the author's anatomical researches on the condition of the fœtal heart at various times, as upon them his peculiar opinions on fœtal circulation are founded.

1. *Insertion of the Vena Cava Inferior into the Heart.*—Originally both auricles form a simple cavity, which, in fact, is nothing but a dilatation of the vena cava inferior; a duplicature of this vein begins gradually to be formed, and thus the simple cavity is divided in two. This duplicature represents in the right auricle the Eustachian, and in the left the valve of the foramen ovale. By the latter valve, the opening of the vein into the left auricle is gradually obliterated, while the opening into the right auricle enlarges in the same proportion.

2. *The Foramen Ovale* is, consequently, not to be considered as an aperture in the septum auriculorum, but as a prolongation of the venous coats; it gradually turns from the right to the left, in the same proportion as the right opening of the vein enlarges, and the left one contracts.

3. *The Pulmonary Artery, Ductus Arteriosus,* and *Aorta Descendens,* are to be considered as the abdominal aorta; whereas, that arising from the left ventricle exhibits the aorta cerebralis, so that in the fœtus there exists *two aortas.* This the author clearly proves by anatomical observations; it also bears a great analogy to the distribution of these vessels, as observed in the progression of red-blooded animals.

4. *The Pulmonary Arteries.*—Several physiologists have maintained that, previous to birth, no blood at all is carried through the lungs. Bichat first demonstrated the incorrectness of this assertion, and proved, that in the same proportion as the fœtus approaches to the period of birth, the pulmonary arteries enlarge, and that, considerable time before respiration commences, the blood passes through the lungs. This our author justly tends to confirm, although he seems to overrate the quantity of this blood; the size of the pulmonary arteries is, indeed, considerable; but we cannot from it exclusively infer the quantum of blood which they convey, as this, besides, depends on the velocity of its movement.

5. *The Umbilical Vein and Ductus Venosus.*—With regard to the latter, the author differs from most writers; according to him, it is not a continuation of the umbilical vein, but belongs properly to the vena portæ; the umbilical vein goes into the left branch of the latter, which is commonly called sinus venæ portæ. The vena portæ is ingeniously compared to the pulmonary artery; both convey, originally, no blood to their respective organs; the same relation exists further between the abdominal aorta and the pulmonary artery, as between the vena portæ and the vena cava inferior, and the function of the ductus arteriosus corresponds exactly with that of the ductus venosus.

We are now fully enabled to follow our author in his description of fœtal circulation. In the liver the blood of the umbilical vein is divided into two unequal parts; the larger portion is carried through the substance of the liver, the smaller portion passes through the ductus venosus into the cava inferior, where it is mixed with the blood, which returns from the lower extremities, &c. In the heart the blood of the cava inferior is again divided, one part goes into the left, the other into the right auricle. The contents of the left auricle consist of a large portion of blood from the left opening of the cava inferior, and of a much smaller quantity from the pulmonary veins. The right auricle contains a small quantity of blood from the right opening of the cava inferior, and the blood of the cava superior. In this manner the blood of both auricles is of a similar mixture.

By the systole of the auricles the blood is carried into the ventricles; on their contraction, the blood of the left cavity is conveyed into the aorta ascendens, and its three branches; that of the right ventricle passes through the duct. arter. into the aorta descendens. Both aortæ have no communication between themselves, and the arterial system of the head, and the circulation of the upper extremities, is completely separate from that of the lower half of the body.

The most important difference in the fœtal circulation, from that in the adult, is the *insertion of the cava inferior in both auricles,* and *the double aorta.*

In the fœtus the left opening of the cava inferior performs the same function as the pulmonary veins in the adult; both are, consequently in the fœtus in an opposite proportion, so that previous to the existence of any pulmonary vein, the cava inferior goes entirely into the left auricle, and in the same degree as the pulmonary veins enlarge, the left cava superior diminishes in diameter, and, lastly, disappears entirely.

On the other side, the umbilical artery represents, in the fœtus, the pulmonary artery of the adult, and there exists the same relation between them, as between the left cava inferior and the pulmonary veins; for while the left cava inferior decreases, the right one proportionally enlarges, and so does the quantity of blood conveyed through the right ven-

tricle into the aorta ascendens; from that period the lower extremities begin to be formed, while up to this time the development of the head and the upper extremities prevailed. All these metamorphoses are accompanied by a decrease in the functions of the placenta; and it seems that in the latter period of fœtal life, the liver is in some degree substituted for it, as appears from the beginning secretion of the bile.

Medical and Philosophical Intelligence.

Aneurism of the middle Artery of the Dura Mater. By Dr. Krimer.—The subject of the case had a tumour about the size of a walnut, not painful upon pressure, hard, moveable, and covered by the sound integuments, situated upon the left temple. It arose from a blow upon that part, received about two years before, had slowly attained its present size, and during its progress had been attended with headach. It was supposed to be an encysted tumour, and an operation was proposed, and assented to by the patient. After cutting through the skin, the operator found that the tumour was situated beneath the temporal muscle, which was divided, carefully avoiding the external temporal artery. The tumour was found attached to the bone by a peduncle about the size of a writing quill; this was divided, and a profuse discharge of arterial blood immediately followed. Alarmed by the hemorrhage, and supposing that he had injured the deep temporal artery, the operator arrested the blood by means of a plug, and attempted to discover the injured vessel; no artery, however, was found within the circumference of an inch of the spot where the blood issued; beyond this distance the deep temporal was found perfectly safe, and upon careful examination, it was discovered that the blood issued from an opening in the bone itself. Not more than a pound and a half of blood was lost, but, notwithstanding all the means employed for his relief, the patient fell into a state of unconsciousness, and died in about two hours afterwards.

Upon examining the sac which formed the tumour, Dr. Krimer ascertained it to be an aneurismal pouch, the opening of which directly corresponded with the situation of the middle artery of the dura mater; the tumour had made its appearance between the squamous portion of the temporal, and the adjoining part of the parietal bones, and from the apparent solidity of its parietes, which were formed by a layer of cellular membrane, by the thickened pericranium, and lined internally by coagulable lymph, could hardly have escaped being taken for an encysted tumour. Permission to open the body being refused, Dr. Krimer broke with a pair of pincers, the inferior and posterior angle of the frontal bone, and a small portion of the temporal, in order to examine, as far as possible, the condition of the internal parts; the bone was found reduced to the third of a line in thickness, and was quite flexible around the opening; for the distance of about three-fourths of an inch, the middle artery of the dura mater was so much dilated as to equal the finger in size, and must have exerted constant pressure on the brain; the pia mater, covered at this spot by an exudation of coagulable lymph, was strongly adherent to the dura mater. The brain itself appeared sound; about an ounce and a half of blood was found in the interior of the cranium, and the extravasation appeared to extend even to the base of this cavity; to the compression thereby induced, Dr. Krimer attributes the death of the patient.—*Jour. des Progres, &c. from Graëfe and Walther's Journal.*

Croton Oil.—It is known to most practitioners in this country that the purgative we possess in the Croton Oil is, in many cases, a powerful but dangerous remedy—to few that it may in any case be administered as a safe and gentle one. The object of this communication is to point out the form in which it can be best administered, to explain its operation, and the cases and constitutions in which it can be safely applied.

As far as my experience with it has gone, I have been decidedly led to prefer its exhibition in the form of pill, combined with compound extract of colocynth or extract of rhubarb, and a little oil of cinnamon. This form is much less objectionable than a fluid preparation, the acidity of which causes considerable uneasiness in the fauces, œsophagus, and stomach. The oil of cinnamon covers its disagreeable and nauseous smell. It may in some cases be employed advantageously mixed with gruel, and used as an enema. The quantity contained in the pill should vary from half a drop to two drops, and no more. The enema might, for an adult, contain two drops.

In about an hour or two after its passage into the stomach the patient usually complains of pain in that organ, accompanied with languor and lassitude, and soon afterwards nausea, retching, and vomiting. The vomiting is not an invariable effect. The pain extends to the abdomen, and becomes true griping, and the bowels are evacuated freely and copiously. Its operation is completed most commonly in three hours from the time of taking it: it appears to have the power of

completely clearing the alimentary canal of all that it had previously contained.

The cases and constitutions to which it is most adapted are those of obstinate constipation, without mechanical obstruction, in robust habits. It must have occurred to all in the practice of medicine to have met with constipation where our ordinary purgatives had no power whatever: in such cases the Croton is invaluable. It may be given to women in the constipation of pregnancy; but the practitioner should be aware that, in such cases, prudence should point out the greatest caution.*—*Lond. Med. Gaz.*

Effects of Galvanism upon the Nerves.—Among the numerous experiments which have been instituted, to prove the analogy of galvanism and nervous influence, those of Professor Weinhold are not the least interesting or curious—the following are some of the most remarkable.

He decapitated a cat, and after the pulse and muscular action had entirely ceased, removed the spinal marrow, and filled the vertebral canal with an amalgam of mercury, zinc and silver. Immediately the pulsation of the arteries returned, and muscular contractions were excited, which could not be distinguished from those produced through the influence of the spinal marrow; the animal began to jump, and did not cease till it had made several leaps. When the irritability appeared to be exhausted, Professor Weinhold, by means of a metallic arc, established a communication between the heart, voluntary muscles, and the artificial medullary substance, and again excited general contractions, which, however, were weaker than the first.

The cranium and vertebral canal of another cat which no longer evinced signs of life, was filled with the same amalgam, and for the space of about twenty minutes, such a state of vital tension (tension vitale) was induced, that the animal raised its head, opened its eyes, looked fixedly, attempted to walk, endeavoured to raise itself after having fallen several times, and finally fell to rise no more. The circulation and pulse were very active during all this time, and continued a quarter of an hour after the thorax and abdomen had been opened. The secretion of the gastric juice was evidently greater than ordinary; the animal heat was perfectly re-established.

The Professor filled also the cranium of a dog with the above mentioned amalgam, and afterwards examined the principal functions of the senses; the pupil still preserved its power of contraction, the animal evinced a desire of avoiding the light when a candle was brought near him, and appeared to listen when the table was struck with a key.

Weinhold has also observed that sparks were evolved from the two extremities of a nerve divided transversely, when the divided ends were approximated to each other. He cut across the crural nerve of a cat, placed the extremities at the distance of a line, and made a communication by means of a metallic arc; at the moment the circle was complete, he saw at each extremity of the nerve a luminous point, which, however, did not pass from one to the other.

The hypothesis of a nervous atmosphere has been completely overturned by the experiment of Weinhold, in which, after having divided the crural nerve, he could not excite contractions of the muscles of the leg by means of galvanism, although the extremities of the nerve were placed at the distance of a line, and even of a fourth of a line. A ligature even, placed upon a nerve, prevents the transmission of galvanism. He observes, moreover, that the nervous pulp is the sole conductor of the galvanic fluid, while the coat is altogether destitute of that faculty.

Weinhold has also investigated the material changes which take place in the nervous system during the action of galvanism. Having isolated the crural nerve of a frog, he observed that the medulla of the nerve, which was almost transparent, shrunk during the contraction of the muscles excited by the galvanic irritation, and that this shrinking alternated with the dilatation. He exposed the tracheal nerve of a rabbit, and observed that after having excited twenty or thirty rapid contractions of the extremities by means of the galvanic pile, the nerve diminished in volume, lost its cylindrical form, and ultimately became a simple, white, and compressed tube. This loss of substance of the nervous medulla during the action of the nerves, was, in the space of twenty or twenty-five minutes repaired by the augmentation of the pulsations of the heart coinciding with the violent contractions of the muscles, so that after a time, the nerve had recovered its cylindrical form. When, on the contrary, the heart had been removed, and the reparation of the nervous substance could not be effected through the medium of the circulation, the nerve did not recover its primitive form. The same loss of substance was also observed in the portion of the spinal marrow which gives rise to the

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* The tree yielding this oil has been recently discovered in Peru, by our countryman, Dr. Burroughs, and he has transmitted to the United States, a specimen of the oil obtained by cold expression, which he thinks preferable to that brought from the East Indies, inasmuch as the latter is always obtained by the aid of heat, and is frequently adulterated. He states, that he has given it in doses of from two to three drops, in the fever of the coast, and in this quantity has always found it to prove a certain, prompt, and safe purgative: he has never seen it productive of unpleasant consequences--in a full dose it sometimes occasions a slight sensation of heat about the fauces and at the pit of the stomach, which, however, goes off very soon after the bowels have been moved. A number of the beans accompanied the specimen.—*Ed. Jour. of For. Med.*

nerves of the thoracic extremities, when the muscles of these extremities were thrown into contraction by the violent and continued action of a galvanic pile upon their nerves. During the action of the nerve, not only the quantity of the nervous substance diminished, but even its consistence. When a nerve was cut across and long subjected to the galvanic influence, he observed that the medulla became progressively softer, and finally distilled *guttatim* from the extremity of the divided trunk.—*Jour. des Progres, &c.*

On the Medicinal Properties of Taraxacum. By Mr. HOULTON.—The most uniform and active preparation of this plant, I believe, may be obtained by carefully evaporating spontaneously* the expressed juice of the roots taken up in August and September. The extract formed in this manner I have found to be a valuable medicine, both in my own person and in practice, and have the concurrent testimony of practitioners of different departments of the profession who have used it with success. A physician, who had been long resident in India, observed to me, after taking some of the extract prepared as above stated, "I have never before found any benefit from taraxacum." It is a tenacious, saponaceous mass, not ductile; it keeps remarkably well if evaporated sufficiently. It is a valuable anodyne, deobstruent, slightly aperient and diuretic. In some cases of chronic diarrhœa it has soothed the bowels, and has given that relief which no other medicine was found to afford.

In cases of chronic disorder of the digestive organs, not produced by intemperance, its efficacy is frequently very decided. In visceral derangements from intemperance I have not found it of much service; but in females, and other persons of sober habits and of studious and sedentary pursuits, it has been very beneficial, increasing the flow of bile, and allaying that uneasiness which the dyspeptic frequently experience about the hepatic region. If practitioners interested in the advancement of pharmacology and therapeutics, would employ the taraxacum in the form here proposed, I feel confident that a proper estimate of its virtues would be ascertained, and that the opinions of the filii apollinis would, respecting this medicine, cease to be discordant.—*Lond. Med. and Surg. Jour.*

Hepatitis and Abscess of the Liver produced by Traumatic Lesions.—The Ephérmérides of Montpellier for March, 1828, contains a series of cases of hepatitis and abscess of the liver, all consecutive of traumatic lesions, collected in the practice of Professor Lallemand. The frequency of these affections of the liver after injury of the brain, had long been known, but physicians had limited themselves to the observance of the connexion between these two organs, up to the period when M. Velpeau further generalized the question, and demonstrated in a work on tuberculous abscesses occurring after great operations, or profuse suppurations, that the liver and lungs are the organs in which these abscesses are especially developed. The following cases are adduced by the physician last mentioned, in corroboration of this opinion.

A soldier, æt. 24, who had been subject to epilepsy for fifteen years, received a wound on the head, in consequence of a fall while in a state of intoxication. He complained of pain in the head, for which he was bled; the abdomen became painful, and recourse was again had to antiphlogistic remedies. He appeared convalescent, when one evening, after exposure to the cool air of the yard of the hospital, he was attacked with erysipelas of the face, pain and tension in the right hypochondrium, with a general icteric tinge. Auscultation revealed, besides the affection of the liver, pleuro-pneumony of the right, and pleurisy of the left side. The disease continued its course, uncontrolled by the remedies employed; the brain became affected, and the patient died. In the liver, which was covered with yellowish spots, surrounded with black areolæ, abscesses were found with a membranous lining; its substance generally was softened; the condition of the lungs confirmed the diagnosis; the brain was injected.

Another soldier, æt. 22, had a jaundiced hue, with tension of the right hypochondrium and vomiting, in consequence of having undergone a very painful operation for the cure of fistula in perineo. These primary symptoms ceased when the catheter was withdrawn, and again recurred upon its re-introduction. Nausea now supervened, with vomiting of thick, greenish bile; the skin resumed its jaundiced tint; the features were sunken; pulse contracted; the right hypochondrium excessively painful on the slightest touch; irregular shiverings, and violent pain in the right knee. The patient died.

The liver was found in the same condition as in the preceding case; the other viscera were sound, but purulent matter was found in the right tibio-femoral articulation. It rarely happens, observes the narrator of the case, that patients die of an uncomplicated disease; most commonly the affection of an important organ induces that of several others; here the viscera escaped, but the articulations were attacked. In this instance there was a singular coincidence between the introduction of the catheter and the development of the symptoms of hepatitis. (Vide the works of MM. Dumas and Prevost, and the experiments of M. Simon De Metz.*)

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* This is effected by placing it in shallow vessels, exposed to a current of dry air, or if placed in a situation artificially warmed in wet seasons, a similar preparation will be produced, one pound of root yielding two ounces of extract.

* Journal of Foreign Medicine, Vol. I. page 399.

In the third case, the abscess of the liver was the consequence of unavailing attempts at lithotrity, during which a portion of the mucous membrane of the bladder was removed in the grasp of the instrument. The symptoms were not so well marked as in the preceding cases, there was neither vomiting nor tumefaction of the right hypochondrium, but only a light icteric tint; acute pain in the lumbar and gluteal regions of the right side and internal part of the corresponding knee; at a later period, infiltration of the whole right inferior extremity, and almost complete suppression of urine.

In these three cases, the substance of the liver evinced different degrees of inflammation; its tissue, softer than natural, was in some places of a deep red colour. Professor Lallemand has long considered softening of the liver as a proof, and a consequence of inflammation of this organ. At a greater depth the pus appeared diffused through its substance, and flowed guttatim when pressure was made upon it; still deeper, genuine abscesses were found. The progress of inflammation, observes M. Velpeau, may thus be traced; 1st, by the softening and redness; 2d, by the purulent infiltration; 3d, by the formation of abscesses. If abscesses, so frequent in the liver, are rarely found in the lungs, this arises from the difference of structure of the respective organs. The areolar tissue of the lungs is not so well adapted to the formation of purulent collections, the pus having a greater tendency to infiltrate its substance. They are so rare, that both Broussais and Bayle deny their existence. Professor Lallemand has seen two examples.—*Journal des Progres, &c.*

Case of Poisoning by Belladonna, followed by Scarlatina. By M. Jolly.—M. N——, æt. 46, took by mistake forty-four grains of the powdered plant; about an hour afterwards, he was attacked with violent headach, seated chiefly about the orbital fossæ, and soon followed by excessive redness of the eyes and face, which gradually extended over the whole surface of the body. The cutis presented a uniform red colour, exactly resembling that observed in scarlatina; moreover, his throat was of a deep red colour, and the seat of an acute sensation of heat, which appeared to extend throughout the alimentary canal. A circumstance not less remarkable, was the great irritation of the urinary passages, and especially of the neck of the bladder; the patient, in the midst of a loquacious delirium, which turned principally upon the pain which he experienced in this part, was continually making efforts to evacuate his urine, which was very red and bloody, and came away guttatim. He was bled largely, and demulcent beverages and emollient enemata, frequently repeated, were directed; with fomentations to the abdomen. The irritation of the bladder continuing, twenty leeches were applied to the hypogastrium, and after a few hours some relief was obtained. The patient slept during the night, and the following morning complained only of a sensation of general *malaise*, which soon disappeared.

Among the reflections arising from this case, there is one to which it may be well to direct the attention of physicians; it is evident that the principal symptoms were those which characterize scarlatina; the cephalalgia, the uniform scarlet redness which showed itself successively upon the whole surface of the body, the angina which preceded, and the inflammation of the digestive and urinary passages which accompanied it, would seem in fact, to constitute a kind of artificial scarlatina. On several occasions, observes M. Jolly, I have seen the powder, and especially the extract of belladonna, produce the same scarlet redness of the skin, but never before had I observed this phenomenon carried to so great an extent, or accompanied by the other symptoms which I have mentioned.

M. Jolly does not infer from this case, that there is an identity of nature between the artificial scarlatina, produced by belladonna, and that arising naturally; still less would he attribute to the former, a preservative property against the latter; he has, however, deemed the fact worthy of record, at a time that such a doctrine has recently been promulgated by the German physicians.—*Nouvelle Bibliotheque Medicale.*

Ossification of the Peritoneal Coat of the Liver. By M. Robarts.—Dr. Baillie has recorded instances where the peritoneal coverings of the spleen and liver were converted into cartilage, more especially the former; and quotes a case from Morgagni where laminæ of bone were found in the midst of it. His words are as follow:—"I have also seen in some instances small spots of cartilage over the whole surface of the spleen. It would appear that ossifications are sometimes found in this cartilage; but in the cases which have come under my own observation, bony matter was not to be observed."

Now, in this case which I opened, the peritoneal covering of the liver was not studded with small spots of cartilage, but converted into one mass of it, being at the thinner parts one-eighth of an inch in thickness, and in many places half an inch; and in the midst of it were several scales of bone, one as large as a half-crown piece.

This is worthy of remark, as indicating that bone is one of the ulterior products of inflammation in serous membranes, and not, as Baillie suggests, a natural process misplaced. In this instance the peritoneum lining the flank was thickened, showing inflammation in its first stage; that covering the liver was cartilaginous, showing it in its second; and some portions of this last were ossified, showing it in its third.

It will also illustrate Dr. Ayre's Pathological Views of Dropsy: inflammation having arisen in a chronic form in the liver (which in this case had a granular appearance,) extended

to its peritoneal covering, and thence throughout the sac generally; thus displaying in different parts the various duration of the inflammatory process.—*Lond. Med. Gaz.*

Rupture of the Uterus at the time of Quickening. By Dr. Else.—Mrs. ——, æt. 20, lost her life under the following circumstances:—She had been married about fifteen months, and, until the time of her conception, had enjoyed tolerable health; but since that period had suffered considerably from deep-seated pain in the back and uterine region, together with other symptoms threatening abortion.

Before her marriage, and up to the time of conception, she had experienced an unusual degree of pain at each menstrual period; and the catamenial discharge was exceedingly scanty. Her death appeared in some measure accelerated by an excursion to Greenwich, in company with her husband, as shortly after her arrival there she was attacked with vomiting and syncope, and in less than an hour she ceased to exist.

Upon examination it was discovered that a rent of about five inches in length had taken place in the uterus, extending itself from the cervix upwards at its anterior part, and rupturing a portion of the placenta. The fœtus lay in front of the uterus, enveloped by its internal membrane, and surrounded by coagulated blood, a quantity of which was also found between the intestines and in the cavity of the pelvis. The uterus itself was covered with dark-coloured spots, and easy lacerable; the ovaries were also in a state of disease—the one containing hydatids, the other with the same dark-coloured spots as the uterus. The fœtus appeared healthy, and is supposed by its movements to have caused the rupture of the uterus.—*Lond. Medical Gazette.*

Gangrene of the Skin of the Lower Extremity.—Professor Graefe has recently published the following interesting case, extracted from an official report, by Dr. Wassoefuhr, physician general to a military division. A musketeer was twice attacked in the course of last year with intermittent fever, and on both occasions completely recovered. In November he suddenly experienced a sensation of heat, very soon followed by pain, in the loins and lower extremities, which at the same time, began to tumefy. Shortly afterwards the skin of the latter was covered with spots of a blackish blue colour, a nervous fever supervened, the spots enlarged, united, and finally presented a gangrenous character, involving a great part of the integuments, but not passing beyond them. In addition to these symptoms, hydrothorax and ascites made their appearance, and the patient died on the seventeenth day of the disease.

On opening the body, the liver and spleen were found diseased; the latter especially, was greatly disorganized, its volume was considerable, and it weighed two pounds and a half. It is evident, therefore, that the gangrenous affection of the extremities depended upon the internal disease.—*Graefe and Walther's Journal.*

Suppuration of the Spleen.—The following case is recorded in the *Osservatore Medico*, published at Naples. A miller, æt. 29, had for some time been affected with an obstruction of the spleen, consequent upon intermittent fever. Subsequently to some excesses in diet and exercise, the tumour increased in volume, and became more painful, presenting all the characters of well marked splenitis. Notwithstanding the repeated application of leeches, the employment of venesection, purgatives, and tartarized antimony, the disease continued its course unchecked; the tension of the hypochondrium increased; the pain became more severe, and was accompanied by shivering followed by heat, nocturnal sweats, &c. The induration of the spleen now disappeared, that organ increased in volume, and became softened at its inferior portion, indicating the existence of the suppurative process. Poultices were applied, and at the expiration of six days the fluctuation was so distinct, that it was decided to give vent to the matter by means of a trocar, which was accordingly introduced at the distance of about four inches from the linea alba. About three pounds of thick, fœtid pus, of a dirty-white colour at first, and afterwards reddish, immediately flowed through the instrument. Great relief followed the operation; the wound, which was maintained open for several days, closed in less than a fortnight, and the patient entirely recovered.

Extirpation of the Parotid Gland.—This operation has recently been performed in two cases of scirrhus, by Mr. Gensoul, of Lyons. During the first operation eleven arteries were tied, the external carotid included. The wound healed within three months, and the patient at this time seemed perfectly cured. After six months he died, from frequent dietetic excesses; and the examination exhibited several disorganizations of the liver and the stomach, which, entirely independent of the previous operation, accounted for his death. No trace of the parotid was found. At the second operation, the external carotid was also tied, and the internal carotid, jugular vein, and the pneumo-gastric nerve, were laid bare up to their entrance into the skull. The patient was perfectly cured, and enjoyed good health for three years afterwards, except a paralytic affection of the left side of the face, the portio dura of the seventh nerve having been divided during the operation.—*Lancet.*

On the Advantages of Graduated Compression in Ascites.—A woman was admitted into the Clinical Institute of Parma, labouring

under ascites, which commenced several months before, and appeared to have been the consequence of peritonitis following difficult labour. The patient was much debilitated, had a slow fever, with great disturbance of the digestive functions; the urine was in small quantity and turbid, little thirst, great emaciation, &c. Squills, drastic purgatives, mercurials, &c., had been tried ineffectually. Professor Speranza did not perform the operation of paracentesis, not entertaining a very favourable opinion of the operation in any case, and from the state of the patient, deeming it particularly inapplicable in the present instance. The observations of Jenniker, Godelle, Recamier, and more recently of Moulon, induced him to employ graduated compression, by means of Monro's bandage. The consequence was a great increase in the quantity of urine, which became more and more perceptible, till ultimately it amounted to more than fifteen pounds in twenty-four hours. In the space of three weeks the swelling of the abdomen had entirely subsided. Under the use of squills, the sulphate of iron, and a more nourishing diet, the fever disappeared, and the patient left the hospital in a state of perfect health.—*Annali di Medicina.*

Ablation of the Penis by means of Ligature; by Dr. Binet.—The patient, an old man, æt. 62, had long laboured under extensive disease of the penis, which involved the whole of that organ, from its extremity to some distance below the symphysis, and rendered its removal necessary. The extent of the disease, the age and debilitated condition of the patient, the dread of hemorrhage, &c., led to the employment of the ligature, or constrictor of Dr. Mayor, recommended by that gentleman for the removal of certain tumours, &c. in preference to the scalpel. The following is the account of the operation.

An incision was made through the sound skin around the root of the penis, and the integuments separated to some distance beyond this part, by means of the fingers, assisted by the bistoury to divide some aponeurotic fibres and the suspensory ligament. In this stage of the operation four small arteries were divided, and immediately secured. The constrictor was then placed as far down as possible upon the penis, at the point of bifurcation of the corpora cavernosa, which was easily effected by drawing the organ forward; an incision four lines in length was then made in front of the constrictor, laying open the urethra, and permitting the introduction of a short silver tube, which was passed about an inch beyond the constrictor, and the instrument tightened as much as the patient could readily bear; the physician proposing to make still greater constriction on the morrow. Little pain followed the operation; the urine flowed through the tube, and in the course of three or four days, the whole of the penis included by the ligature, was thrown off in a state of mortification. The patient entirely recovered. —*Revue Médicale.*

Calculous Concretion in the Lacrymal Sac; by Dr. Krimer.—A woman, æt. 32, had been labouring under fistula lacrymalis for the space of nine months, when she consulted Dr. Krimer. This gentleman, introducing a probe into the fistula, found that the sac was not ulcerated, but that the obstruction arose from a hard body, which he supposed to be an osseous exudation, situated in the nasal canal. He attempted to overcome the obstruction by means of a pointed probe, but not succeeding, withdrew the instrument, in doing which, he met with considerable resistance, and was not a little surprised to find, attached to its point, a calculous concretion about the size of a small pea. The obstruction of the canal was now entirely removed; catgut bougies were introduced, and the cure was completed after the lapse of fifteen days. —*Graëfe and Walther's Journal.*

Meliceris.—A tumour about the size of an orange, situated upon the left cheek, was removed by Professor Weinhold, of Halle; a rapid and complete cicatrization followed its ablation. Such an excessively fetid odour was exhaled from the contents of the tumour, that the surgeon and his assistants were affected with nausea. Fourteen days afterwards the patient was attacked with intermittent fever, which resisted the most powerful remedies during six weeks, and did not yield until there appeared upon the left leg, an ulcer which secreted a matter, in odour perfectly similar to that of the contents of the tumour.—*Bibliothek der practishen Heilkunde.*

Traces of Iodine in the Blood drawn from the vein of a person, who had employed, for some time, frictions with an ointment made from this substance; by M. Bennerscheidt.—No vestige of iodine was found in the serum of the blood, but traces of it were observed in the crassamentum, too slightly marked, however, to permit it to be obtained in an isolated state. The starch merely contracted a light blue shade. M. B. proposes to repeat his experiments upon larger quantities of blood.—*Archiv. des Apotheker Vereins, &c.*

Vegetable Gelatine, and Albumen.—M. Berzelius has lately examined gluten, and says that the gliadine and zymoma of Taddei are nothing else than the well known and ordinary principles of vegetables named above. Boil gluten with successive portions of alcohol until the latter ceases to become turbid upon cooling; mix these solutions with water, and distill; as the aqueous residuum cools, a glutinous coherent mass will separate, resembling gluten. It is *vegetable gelatine*, and the same substance as that separated by Einhof's

process from barley, &c. The substance insoluble in alcohol is vegetable albumen.

Vegetable gelatine is grayish-yellow in colour, adhesive, glutinous and elastic, having no taste, but a peculiar odour. It dries into a transparent, shining substance. It dissolves in alcohol; if cold alcohol be used, a viscid foreign substance is separated, not gelatine. It dissolves in vinegar, leaving also a viscid insoluble matter; when precipitated by an alkali, it resumes its viscid state. The mineral acids, with the exception of the phosphoric, form glutinous compounds insoluble until the excess of acid has been removed. This principle combines with and neutralizes alkalies, forming solutions, which, when evaporated, yield a transparent matter. Earths and oxides form insoluble compounds.

Vegetable albumen is almost perfect in its resemblance to white of egg. It dissolves in alkalies, and when in excess, the solutions are neutral. It then coagulates slightly by heat, but the principal part is retained in solution; it combines with acids, and when exactly saturated the substance remains soluble, but excess of acid (except the acetic and phosphoric) precipitates it. Before the action of potash, the vegetable albumen dissolves feebly in vinegar or phosphoric acid, but by ebullition with these acids, it forms a transparent colourless jelly of considerable volume.

The azoted principle contained in emulsive seeds has been considered analogous to the coagulum of milk. Souberian has shown that that from almonds has all the properties of white of egg; it is, in fact, the same substance as vegetable albumen.—*Ann. de Chimie*, xxxvii. 215.

Reduction of Sulphuret of Arsenic.—Sulphuret of arsenic is occasionally required to be reduced, when in very small quantities, in medico-chemical investigations. Berzelius remarks, that it may frequently be successfully performed, by putting it at the bottom of a small glass-tube, placing a small piece of steel-wire before it, and subliming it over the latter; the iron takes the sulphur; the arsenic condenses a little in advance. When the quantities are very small, this process sometimes fails; then Berzelius recommends the following:—The sulphuret is to be introduced into an open quill glass-tube, about four or five inches long, and being held obliquely, thus \, is to be heated by a spirit-lamp, so that the hottest part shall be a little above the sulphuret, and the vapour be obliged to pass by it; the operation should be conducted slowly; the sulphur will burn into sulphurous acid and escape, and the arsenic into arsenious acid, which will condense in the upper cool part in crystals. The tube is then to be softened in the lamp, and drawn out below the arsenious acid; a little piece of charcoal is to be introduced, and then the arsenious acid passed across it in vapour, to the narrow elongated part of the tube; it will be reduced by the charcoal in its passage, and metallic arsenic will appear. This process never fails. —*Annalen der Physik.*

Decomposition of Ammonia by Metals.—M. Savart found that 141·90 grains of thin copper wire became 142·382 grains, or acquired an increase of 0·472 in weight, when used for four hours to decompose ammonia: as the wire was in a slight degree oxidized, the experiment was repeated; and when every precaution was employed, the increase amounted to $\frac{1}{275}$, and 0·105 of an unknown substance was absorbed by the copper, and its specific gravity was diminished from 8·8659 to 7·7919.

Iron also increases in weight, and diminishes in specific gravity by similar treatment, and will strike fire with flint like ordinary steel.—*Ann. de Chimie.*

Effect of Elevation upon the Pulse and Breathing.—Dr. Brunner, in ascending Mount Etna, in 1826, found that at Nicolosi, 3200 feet above the level of the sea, his pulse was 72; at Casa Gemmellara, 9300 feet high, it was 80; and at the summit of the mountain, 10,152 feet, it was 84; his natural pulse on the plain being 62–63. Notwithstanding the tenuity of the air at the above elevation, he experienced no inconvenience in respiration. These observations correspond to some made by Dr. Parrot on the Pyrenees.—*Foriep's Notizen*, No. 6.

Native Iodide of Mercury.—M. Del Rio has already mentioned that he has discovered iodide of silver in America, and he has mentioned its locality. He has since discovered another iodide; and he is of opinion that the metal in combination with it is mercury. It perfectly resembles dark-coloured cinnabar, except that its colour is deeper and its streak paler; it is however certain, that it accompanies and earthy iodide, which M. Del Rio believes to be the metal of magnesia mineralized by iodine.—*Hensman's Repertoire de Chimie.*

New Vegetable Alkali.—Dr. Nicholas Mill, of Bogota, Columbia, has communicated to the Editor of the Quarterly Journal of Science and Art, that he has discovered a new vegetable alkali in the Quina Blanca of Mutis (Cinchona Ovalifolia, Cinchona Macrocarpa of Vohl,) which he calls Blanquinine, to distinguish it from others, and to convey an idea whence it proceeds. He is now engaged in examining its salts, and he promises to publish, through the same medium, the results of his researches on this interesting substance.

Committee for Investigating the Plague.—*Academie Royale de Medecine.* This body has elected a committee of five members for the study of the plague in the Levant. MM. Panjet and Champollion are amongst the number. They were to embark at the end of last month at Toulon, for Alexandria.—*From La Clinique.*

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